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ABSTRACT

Project Redirection was designed to help pregnant and parenting adolescents progress toward eventual self-sufficiency by linking them with community agencies and volunteers at four geographically and ethnically diverse sites in the United States. Distinctive features of the program include: (1) a broad scope of services including employability training and educational counseling; and (2) the inclusion of paid women drawn from the local community to act as primary supports to the teens and help them achieve short-term goals. This report discusses impact findings two years after the teens first enrolled in Project Redirection. The hypothesis tested is that participants in the program will experience better education, employment, family planning, and health outcomes than a group of similar non-participants. Participation in Project Redirection did result in some short-term improvements in most subgroups and for most outcomes. By the end of the study, however, when teens were out of the program, differences between the experimental and comparison groups had largely disappeared, except for certain subgroups. The report concludes with a summary of lessons to be considered by policymakers, program planners, and funding agencies. (LHW)

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MANPOWER DEMONSTRATION
RESEARCH CORPORATION

FINAL IMPACTS FROM PROJECT REDIRECTION

A Program for Pregnant and Parenting Teens

DENISE POLIT
JANET KAHN
DAVID STEVENS

APRIL 1985

UD 025 190

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FINAL IMPACTS
FROM PROJECT REDIRECTION
A PROGRAM FOR PREGNANT AND PARENTING TEENS

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Manpower Demonstration
Research Corporation

April 1985

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The Authors

PREFACE

This document is the final impact report on Project Redirection, a service program directed to young, low-income pregnant and parenting adolescents. Project Redirection was primarily distinguished from other programs for teen parents in three ways: the program focused on a highly disadvantaged group of teens, it offered a comprehensive range of services, and it used two innovative features -- community women, older volunteers who acted as role models or guides to teens, and the Individual Participant Plan, a document signed by each teen specifying her plan for taking part in the services. The planners and program operators hoped that Project Redirection's assistance in helping the teens use a wide array of services would lead to subsequent changes in the participants' educational and employment behavior, improved maternal and infant health, acquisition of life management skills and delay of subsequent pregnancy.

Since 1980, the nonprofit Manpower Demonstration Research Corporation (MDRC) has had responsibility for overseeing the operations and research on Project Redirection. The Project Redirection study was designed to address a number of questions:

- Was the program feasible to operate? What were its costs? (the implementation study)
- How did the background and life circumstances of participants influence the behavior the program sought to change? (the ethnographic study)
- What was the effect of Project Redirection on teens' contraceptive, childbearing, educational and employment-related behaviors? (the impact study)

Earlier reports covered the implementation and ethnographic issues.

In addition, studies released in 1982 and 1983 examined the needs and characteristics of eligible teens before program inception and Redirection's effects on these teens 12 months after enrollment. This report presents the complete set of findings from the impact study, primarily data analyzed 24 months after the teens' enrollment. A monograph, "The Challenge of Serving Pregnant and Parenting Teens: Lessons from Project Redirection," summarizes all of the research findings from the demonstration and presents MDRC's perceptions of the Redirection experience.

The design of the impact study for Project Redirection, conducted by The American Institutes for Research in the Behavioral Sciences and by Humanalysis, Inc., sought to overcome many of the drawbacks of earlier evaluations of service programs for adolescent parents by including large sample sizes, a follow-up period considered adequate to obtain a reliable measure of the program's effectiveness, and a comparison group against which to measure the achievements of participants. Because it was impossible for local sites to recruit sufficient eligible participants to form both a treatment and a no-treatment group, the comparison group was not obtained by random assignment. Instead, a group of teens who met the program's eligibility requirements, but lived in cities or neighborhoods where Redirection was not offered, served as the basis of comparison.

This report tells a complex story. At 12 months, the Redirection teens' progress in several areas -- family planning, continuation of school and employment activity -- was significantly better than the performance of teens in the comparison group. This suggests that services, when available and used, can make a difference. However, at 24 months, one year after most of the participants had left the program, the Redirection teens behaved no differently on most important measures than did the young women in the comparison group. Project Redirection's measured positive effects were sustained only for certain subgroups.

Several methodological issues should be considered in interpreting these results. For a number of reasons, primarily because both groups of teens made considerable use of services, the impact analysis is a conservative test of a service program for this population. While carefully designed and executed, this study could not, as originally intended, compare the behavior of Redirection teens to that of a group receiving relatively few services. And, even if the preferred methodology -- a comparison of the outcomes of randomly assigned experimental and control groups -- had been utilized, the results probably would have been similar, given the fact that teen programs proliferated around the country during the study period.

Nevertheless, the circumstances of program participants two years after enrollment suggest that the services offered were not enough. At that point, almost half of the Redirection teens were pregnant again, and a similar proportion were neither in school nor working. The report and monograph point to a number of ways in which service programs must be strengthened and extended if they are to make a lasting difference. The Project Redirection findings provide a rich base of knowledge on which to build.

Judith M. Gueron
Executive Vice-President

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EXECUTIVE SUMMARY

Project Redirection was a national demonstration of a comprehensive service program for low-income teenage mothers and mothers-to-be. The original demonstration, on which this study is based, began in four sites in mid-1980 and concluded in 1983. Previous reports covered implementation of the program and its early impacts; this report describes the impacts of the program on its participants at both 12 and 24 months after enrollment.

Some components of the Redirection program were replicated in seven other sites in 1983, but the participants in these additional sites are not part of the research sample for this report. The operational aspects of this replication demonstration will be covered in a separate report.

The Project Redirection Program

Project Redirection was a multi-site demonstration that either provided directly or brokered services designed to redirect the lives of young, disadvantaged women toward eventual economic self-sufficiency. To be eligible for the program, teens had to be 17 years old or younger; pregnant or a parent; without a diploma or GED certificate; and usually living in a low-income household in which one or more persons received or was eligible to receive welfare. The program was implemented in

community-based organizations in Boston, Massachusetts; New York (Harlem), New York; Phoenix, Arizona; and Riverside, California.

Several features of Project Redirection distinguish it from many programs now available to young parents. First, Redirection coordinated many different kinds of services, including employability training, by forming linkages with other service providers in the community. Second, three mechanisms were used to help teens take advantage of these services: community women, Individual Participant Plans (IPPs), and peer group sessions. The community women were volunteers drawn from the local community who served as role models and supports to the teens as they worked toward their personal goals. The community women, as well as other program staff, assisted the teens in developing their IPPs, which specified on an individualized basis the services and activities teens would need in order to work toward self-sufficiency. The peer group meetings offered opportunities for social support, mutual problem-solving, and affirmation of program goals.

The demonstration had an extensive research plan, including analyses of the program's effects (impacts), implementation, program costs, and an in-depth ethnographic study. The demonstration reports, listed at the conclusion of this report, are available on request.

The Impact Analysis

The impact analysis was designed to evaluate the effects of Project Redirection on its enrollees. The general hypothesis being tested was that program participants would experience better educational, employment, family planning, and health outcomes than a group of similar nonparticipants.

In the impact analysis, longitudinal data were collected from a sample of 305 program participants (experimental teens) and 370 comparison group members, who were teens eligible for the program but residing in other communities. This study makes use of data covering three points in time: baseline (the time of program enrollment) and 12 and 24 months later.

Because the groups were not determined by random assignment, a major concern in the impact analysis was whether these two groups were as similar as possible on all characteristics at the outset of the study. Information obtained at the initial interview indicates that there was, in fact, an important difference. Comparison group teens were substantially more likely to be enrolled in school than experimental teens, suggesting that comparison teens may have been more motivated overall or from more stable backgrounds. This created a problem in interpreting the study's results.

A second issue that affected the analysis was the growth in the number of programs serving teen parents nationally during the demonstration period. The widespread availability of assistance resulted in a high level of service receipt among comparison teens. Thus, while the impact analysis was originally conceived as a study to shed light on the effectiveness of service provision to teen parents, the information gathered only addresses the question of whether the Project Redirection model, which provided comprehensive services in a supportive environment, was more effective than services that were normally available.

Characteristics of the Research Sample

The initial interviews with both experimental and comparison group members indicated that these teens came from extremely disadvantaged backgrounds. The typical teen was about 16 years old, unmarried, and living in a household that received welfare. At that point, only about half the teens were enrolled in school, and the majority were a year or more behind in grade level. Many had dropped out of school even before their pregnancies. A substantial percentage had had two or more pregnancies, and fewer than half had ever used any form of contraception. The majority had grown up in households headed by mothers who themselves had not finished school and had given birth during their teen years. All but about 10% of the sample were members of ethnic minorities.

Thus, teens in both the experimental and comparison groups represented a target of concern for social intervention. Upon entry into the study, these young women had limited resources. While teens in both groups were from disadvantaged backgrounds, the higher rate of school enrollment among comparison teens may be indicative of more resources (such as motivation or family support) relative to Project Redirection participants.

Program Participation and Service Receipt

By the time of the final interview, all but three teens in the experimental group had terminated from Project Redirection. The mean length of enrollment was almost one year, a longer duration than that reported by most other teen parent programs. The majority of Redirection participants expressed satisfaction with the program, and noted the three program features that most helped them: parenting education, the community woman, and the employment workshops.

Teens in both the experimental and comparison groups reported having received many services during the two-year study period. The most widely used services were medical care (for themselves and their babies), birth control counseling, nutrition and parenting education, and employability training. Significantly greater percentages of experimental than comparison teens received services while enrolled in the program, but service receipt dropped off after program termination. As noted above, comparison teens were also a well-served group. The majority of these teens (54%) reported having been enrolled in teen parent programs in their own communities.

Incremental Impacts

The impact analysis compared experimental and comparison teens on outcomes in the areas of health/fertility, education and employment. Impacts were assessed both at 12 months after enrollment (when the majority of teens were either still in the program or had recently left it) and at 24 months after enrollment (when virtually all teens had ended their program participation).

It is important to note that because of the initial non-equivalence of teens in the two research groups and the high levels of service receipt among comparison teens, the impact analysis provides an extremely conservative test of program impacts. The study does not, in fact, address the issue of the effectiveness of service provision to teen parents compared to no services, but is rather an analysis of the incremental effectiveness of Redirection's coordinating mechanisms.

Despite the conservative nature of the design, the incremental impacts at 12 months after enrollment were diverse and substantial. Relative to the well-served comparison group, Redirection participants showed gains in schooling, employment, and pregnancy prevention. These gains were observed for all participants as well as for most subgroups of experimental teens, such as black and Hispanic teens, older and younger teens, school dropouts and non-dropouts, and teens who entered while pregnant or as mothers. These incremental gains, however, were largely transitory. By 24 months after enrollment, teens in the experimental group were generally similar to comparison group teens on most outcomes.

Table I summarizes incremental impacts for selected outcomes at both 12 and 24 months after enrollment. In each case, the outcome for Project Redirection participants is compared with that for comparison teens, after adjusting for baseline differences. The findings are discussed in greater detail below.

Fertility/Health-Related Outcomes

By the final interview, the majority of teens had gained experience with contraceptives, especially the pill. Nevertheless, birth control was practiced inconsistently by teens in both groups, resulting in high rates of repeat pregnancy during the study period.

By 12 months after enrollment, 17% of the research sample had had a subsequent pregnancy. However, Project Redirection participants experienced a significantly lower rate of repeat pregnancy (14%) than comparison teens (22%). The experimental advantage was particularly strong among black teens, teens who were mothers at baseline, and those who were living in a household receiving Aid to Families with Dependent Children (AFDC) at baseline.

TABLE I

SELECTED IMPACTS OF PROJECT REDIRECTION AT 12 AND 24 MONTHS POST-BASELINE

Outcome Variables	Project Redirection Teens	Comparison Teens	Difference	Percent Increase/ Decrease
A. Fertility				
Rate of Subsequent Pregnancy 12 Months Post-Baseline	14	22	- 8*	-25
Rate of Subsequent Pregnancy 24 Months Post-Baseline	45	49	- 4	- 8
Rate of Subsequent Live Birth 24 Months Post-Baseline	22	29	- 7+	-24
Percent Used Contraceptive at Last Intercourse, 12 Months Post-Baseline	54	45	9*	20
Percent Used Contraceptive at Last Intercourse, 24 Months Post-Baseline	54	54	0	0
B. Education				
Percent in School/Completed, 12 Months Post-Baseline	56	49	7*	14
Percent in School/Completed, 24 Months Post-Baseline	43	43	0	0
Percent Ever Enrolled in School Baseline to 12-Month Interview	75	51	24***	47
Percent Ever Enrolled in School Baseline to 24-Month Interview	87	71	16**	23
C. Employment				
Percent Either in School/Completed or Employed at 24-Month Interview	51	48	3	6
Percent Either in School/Completed or in the Labor Force at 24-Month Interview	74	65	9**	26
Percent Employed, 12 Months Post-Baseline	14	12	2	17
Percent Employed, 24 Months Post-Baseline	15	15	0	0
Percent Ever Employed, Baseline to 12-Month Interview	49	38	11**	29
Percent Ever Employed, Baseline to 24-Month Interview	61	54	7+	13

SOURCE: Tabulations are from AIR interviews with Project Redirection participants and comparison groups members. See Tables 4.4, 4.11, 5.4 and 6.5 in main body of text.

NOTES: All means and percentages have been adjusted statistically for important background characteristics.

+Statistically significant at the .10 level, two-tailed test.
*Statistically significant at the .05 level, two-tailed test.
**Statistically significant at the .01 level, two-tailed test.
***Statistically significant at the .001 level, two-tailed test.

By 24 months after the baseline interview, when experimental teens were no longer in the program, group differences disappeared: 45% of the experimental and 49% of the comparison teens had a subsequent pregnancy. The rate of subsequent live births was 22% and 29% for experimental and comparison teens, respectively. Thus, after two years, participation in Project Redirection was associated with a delay of about two months, on average, in the timing of a repeat pregnancy, but not with an overall reduction in the rate of repeat pregnancy.

An examination of the contraceptive behaviors of these teens provides further evidence that Project Redirection had a positive, but temporary, impact on its participants. At the 12-month interview, significantly more experimental than comparison teens (78% versus 69%) said that they had experience with a medically-prescribed method of contraception--pills, IUDs, and diaphragms--and significantly more had been protected at last intercourse (54% versus 45%). The experimental group advantage disappeared by the 24-month interview for both outcomes. However, at the end of the study, experimental teens did score significantly higher on a test of birth control knowledge.

Health-related pregnancy outcomes for teens who were pregnant during the study were also examined. The overwhelming majority of teens pregnant at baseline (93%) reported having received prenatal care at least five times. The average birthweight of infants born to these teens was just under seven pounds; only 7% of the babies were of low birth weight (under 5.5 pounds). There were no significant group differences on the health outcomes studied. Both experimental and comparison group teens appeared to be receiving above-average medical attention in comparison with national norms for pregnant teenagers.

Educational Outcomes

Generally, the teens in the research sample continued to experience educational deficits during the study period. The majority of teens were in and out of school or other educational programs during the two years and were substantially behind in grade level throughout. Only two-fifths of the sample were in school or had obtained a diploma at the time of the final interview, when these teens were about 18 years old.

Participation in Project Redirection appears to have had a large incremental effect on the teens' educational behavior during the first 12 months of the study. The program was successful in encouraging teens to either return to or stay in school while they remained in the program. Twelve months after enrollment, 56% of the experimental and 49% of the comparison teens were in school or had completed it. A full three-fourths of the Redirection participants had enrolled in an educational program between the baseline and 12-month interviews, compared to only half of the comparison teens. These differences were found in virtually every subgroup.

However, these incremental educational gains were not sustained after participants left the program. At the end of the study, experimental and comparison teens were equally likely to be in school or to have obtained their diploma or GED certificate.

Employment Outcomes

At the 24-month interview, only 15% of the research sample was employed. When school and employment are considered simultaneously, 32% of the sample was neither in school nor in the labor force (employed or looking for work). Teens with a subsequent pregnancy were especially likely to have dropped out of school and the labor force.

Redirection participants (74%) were significantly more likely than comparison teens (65%) to be involved in either school or the labor force at the end of the study. However, the difference is largely attributable to higher percentages of experimental teens looking for work. When only actual employment and school status are considered, the two groups were similar at the end of the study.

Participation in the program was associated with some short-term employment gains that occurred principally during the first 12 months and then disappeared by the end of the study. For example, significantly more Redirection participants (49%) than comparison teens (38%) had worked at some point between the baseline and the 12-month interviews. These gains in work experience did not, however, translate into actual employment at the study's end. Fifteen percent of both experimental and comparison teens were employed at the 24-month interview, net of other factors.

Other Outcomes

In the three major areas in which the teens' behavior was examined (i.e., fertility, education and employment), program effects were generally observed during the initial 12 months and then disappeared by the end of the study. However, program effects on several non-behavioral outcome measures were generally less transitory. At the end of the study, experimental teens had gained significantly more knowledge about job-seeking and other employability skills than comparison teens. Furthermore, Redirection participants scored significantly higher on measures of self-esteem and personal efficacy than comparison teens.

Subgroup Outcomes

While Redirection's incremental impacts for the research sample as a whole largely disappeared by 24 months, certain subgroups of participants showed gains beyond the period of their enrollment in the program. Selected subgroup results are presented in Table II.

Longer-term outcomes in the health/fertility area proved to be most immune to program influence, even within subgroups. By the end of the study, experimental and comparison group teens had similar rates of repeat pregnancy in all but one subgroup: Puerto Rican girls in the experimental group had a significantly lower rate of subsequent pregnancy than those in the comparison group (42% versus 63%). However, as shown in Table II, experimental teens in several subgroups had a lower rate of subsequent live births at the end of the study than their comparison group counterparts, accounted for almost entirely by a delay in the timing of the subsequent pregnancy.

Despite the absence of educational impacts for the aggregated sample, there were certain subgroups for whom significant improvements were observed at the end of the study. Among teens who were school dropouts at baseline, 20% of the Redirection participants but only 11% of the comparison teens had obtained a diploma or GED certificate by the 24-month interview. Participation in the program also seemed to counteract the negative effect of having a repeat pregnancy. Among teens with a pregnancy after enrollment, 20% of the experimental versus 12% of the comparison teens had finished their basic schooling by the end of the study.

Teens who were enrolled for at least 12 months also had favorable educational outcomes. For example, after controlling for background characteristics, 26% of the long-term Redirection enrollees and 20% of the

TABLE II

SELECTED SUBGROUP RESULTS AT 24 MONTHS POST-BASELINE

Outcome Variables	Project Redirection Teens	Comparison Teens	Difference	Percent Increase/ Decrease
A. Fertility				
Percent with a Subsequent Live Birth:				
Teens Not in School at Baseline	32	41	- 9	-16
Teens in AFDC Household, Baseline	21	32	-11*	-26
Teens in Redirection > 12 Months	19	29	-10+	-11
B. Education				
Percent Who Received Diploma/GED Certificate:				
Teens Not in School at Baseline	20	11	9*	82
Teens in AFDC Household, Baseline	19	17	2	12
Teens with a Repeat Pregnancy	20	12	8+	67
Teens in Redirection > 12 Months	26	20	6+	30
C. Employment				
Percent Employed				
Teens Not in School at Baseline	16	11	5	45
Teens in AFDC Household, Baseline	16	10	6*	60
Teens with a Repeat Pregnancy	10	6	4	67
Teens in Redirection > 12 Months	17	15	2	13
D. Education/Employment				
Percent Either in School or Employed				
Teens Not in School at Baseline	36	25	11*	44
Teens in AFDC Household, Baseline	48	44	4	9
Teens Never Employed at Baseline	45	35	10+	29
Teens with a Repeat Pregnancy	44	34	10*	29
Teens in Redirection > 12 Months	57	48	9*	19*

SOURCE: Tabulations are from AIR interviews with Project Redirection participants and comparison groups members. See Tables 4.6, 5.5, 6.6 and 6.8 in main body of text.

NOTES: All means and percentages have been adjusted statistically for important background characteristics.

+Statistically significant at the .10 level, two-tailed test.
*Statistically significant at the .05 level, two-tailed test.

comparison teens had completed school by the 24-month interview. However, teens who remained in the program longer may have been more motivated or faced fewer situational obstacles. Because such unmeasured factors could not be statistically controlled, these impacts may not have resulted from sustained intervention alone and therefore must be cautiously interpreted.

Incremental impacts were also modest in employment outcomes, both for the subgroups and the entire sample. Only experimental teens who were initially living in an AFDC household sustained gains relative to comparison teens at the end of the study: 16% versus 10% were employed. However, when school and work status are considered simultaneously, some longer-term advantages appear to be associated with participation in Project Redirection. For example, Redirection teens who entered the study as school dropouts and those with no prior work experience were significantly more likely to be either working or in school at the time of the 24-month interview. Program impacts were also sustained for teens with a subsequent pregnancy during the two years under study. Some 44% of these Redirection participants but only 34% of their comparison counterparts were either in school or working at the final interview.

Finally, significant improvements in the school/work status of teens were observed among teens enrolled for at least one year, although it is again unclear whether this was caused by the longer intervention or personal traits of the teens themselves. Overall, it appears that the program had the strongest and most long-lasting effects on the most disadvantaged participants.

Conclusions

Due to the nature of this study, many unanswered questions remain about service provision to economically disadvantaged teenage parents. There is evidence that participation in Project Redirection did result in some short-term improvements, even in comparison with similar teens who themselves were fairly well served. These improvements were found in most subgroups and for most outcomes. By the end of the study, however, when teens were no longer in the program, differences between the experimental and comparison groups had largely disappeared, except for certain subgroups. At the final interview, the behavior of the Redirection teens looked very similar to that of comparison teens in terms of subsequent pregnancy, current school enrollment or completion, and employment status.

However, because the implemented design was conservative, it is impossible to determine whether Redirection participants benefited from enrollment in the program in the long run, relative to what would have happened in the program's absence. The 12-month results do suggest that the program was an intervention with some promising features. Nevertheless, regardless of any incremental improvements, the absolute level of continuing disadvantage to teens in both groups suggests that current models of intervention for this population are not adequate. With a repeat pregnancy rate approaching 50% and a school completion rate of only 20%, it is clear that this target population is a serious and ongoing problem for policymakers and service providers concerned with the consequences of teenage parenthood.

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FINAL IMPACTS

FROM PROJECT REDIRECTION

A PROGRAM FOR PREGNANT AND PARENTING TEENS

CHAPTER 1

INTRODUCTION

Project Redirection is a demonstration program of services for low-income teenage mothers and pregnant teenagers. The major purpose of the demonstration is to assess the feasibility and impacts of a comprehensive service program that attempts to "redirect" the lives of young women whose early parenthood places them at high risk to welfare dependency and poverty. Project Redirection's goal is to promote eventual economic self-sufficiency among these young women.

The original Project Redirection demonstration was put into operation in Boston, Massachusetts; New York City (Harlem), New York; Phoenix, Arizona; and Riverside, California. The program, which was implemented in community-based organizations in these four sites, began enrolling participants in mid-1980. The original demonstration was concluded in 1983. The demonstration was expanded to include seven new replication sites in 1983. This document focuses on program impacts during the initial demonstration.

A. Background of the Problem

Premarital pregnancy and out-of-wedlock childbirth among the young are sometimes referred to as being of "epidemic" proportions. Whether or not current rates constitute an epidemic, it is well documented that rates of premarital sexual experience and pregnancy are on the rise in this country. Among the young women who become pregnant, increasing numbers are opting to raise their babies and fewer of them are marrying to escape the stigma of illegitimacy. The result is that there is a growing number of unmarried young women who are raising children on their own. Concern about teenage parents has been fueled not only by the rise in their numbers but also by the mounting evidence that these young women, as a result of their early childbearing, suffer a host of personal, financial, educational and health-related problems (Alan Guttmacher Institute, 1981; Card and Wise, 1978; McCarthy and Radish, 1982; Moore, Hofferth, Caldwell and Waite, 1979; Moore, Simms and Betsey, 1983; National Center for Health Statistics, 1982).

The economic consequences of teenage parenthood have attracted particular concern in this era of fiscal restraint because the economic burden is borne by the public as well as by the teens themselves. Families headed by teenage mothers are especially likely to be on welfare. Over half of all the families receiving Aid to Families with Dependent Children (AFDC) are headed by women who were teenage mothers. Furthermore, teenage mothers are at especially high risk to long-term welfare dependency. A recent study estimated that a non-white high school dropout who becomes an AFDC recipient when she becomes a single mother will average about ten years in her spell of dependency (Bane and Ellwood, 1983).

Research has also shown that, while young women of all ethnic and economic backgrounds become premaritally pregnant, the incidence is particularly high and the consequences particularly severe among economically disadvantaged and minority teens (Moore et al., 1983). Children born to unmarried young mothers are especially likely to grow up in poverty; they are also especially likely to become teenage parents themselves, thereby limiting their opportunity to escape poverty in adulthood. Figure 1.1 summarizes the results of research that has documented the effects of teenage childbearing and the "cycle of poverty" to which it often gives rise.

In recognition of the adverse life outcomes associated with young childbearing, many social interventions have been devised in recent years to reduce the incidence of teenage pregnancy and to offset the early handicaps of those teens who do become pregnant. Project Redirection is one such intervention, aimed in particular at those who are victims of the cycle of poverty. This report describes the program, its participants, and its impacts after several years of operation.

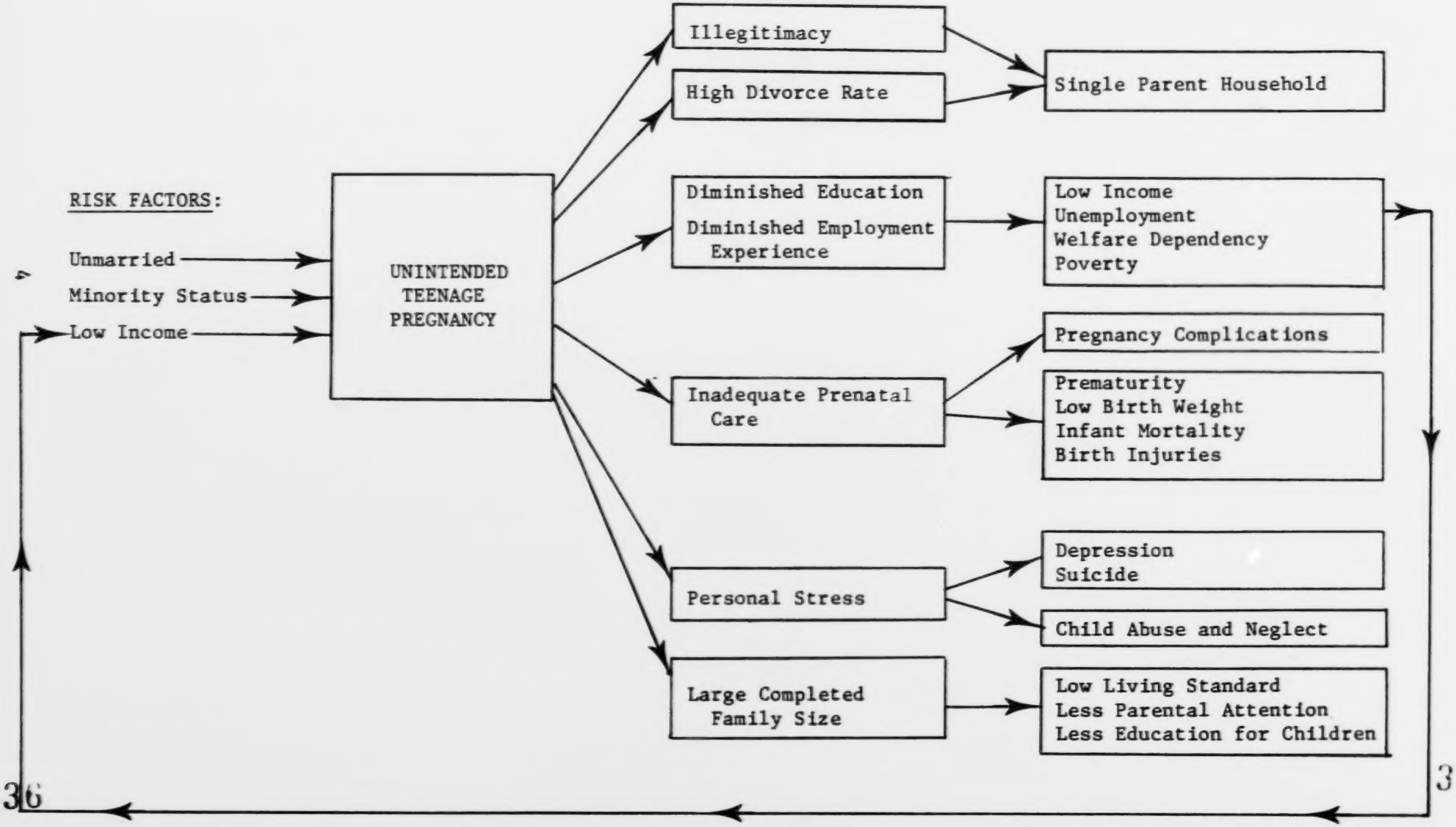
B. The Project Redirection Demonstration

The overall goal of Project Redirection is to redirect the lives of young mothers and mothers-to-be onto a path of long-term economic self-sufficiency. Given the young age of the program's participants, five shorter-term objectives that contributed to the long-range goal were adopted: attainment of a high school diploma or an equivalency (GED) certificate; acquisition of skills and experiences that would enhance their employability; delay of subsequent pregnancies; improved health care for the teens and their infants; and the acquisition of life management skills.

Although there were some inter-site differences in implementing the demonstration, there were many core features that characterized the

FIGURE 1.1

ANTECEDENTS AND CONSEQUENCES OF UNINTENDED TEENAGE PREGNANCIES


RISK FACTORS:
Unmarried
Minority Status
Low Income
UNINTENDED TEENAGE PREGNANCY
Illegitimacy
High Divorce Rate
Diminished Education
Diminished Employment Experience
Inadequate Prenatal Care
Personal Stress
Large Completed Family Size
Single Parent Household
Low Income
Unemployment
Welfare Dependency
Poverty
Pregnancy Complications
Prematurity
Low Birth Weight
Infant Mortality
Birth Injuries
Depression
Suicide
Child Abuse and Neglect
Low Living Standard
Less Parental Attention
Less Education for Children

Project Redirection programs. First, all four sites served specific clientele. Teens eligible to enroll in the program had to be (1) age 17 or younger; (2) pregnant or a mother; (3) without a high school diploma or GED; and (4) receiving welfare or living in a welfare-dependent family (up to 20% could be living in a non-AFDC household whose annual income was below 70% of the lower living standard). A description of the Redirection participants is presented in Chapter 2.

A second core feature was that the Project Redirection program coordinated a comprehensive mix of services. The program model was developed, in part, in response to a growing awareness that existing services for this population were fragmented, inefficient, or inadequate. The National Association Concerned with School-Aged Parents (NACSAP) concluded after surveying 50 service providers in 1977 that "the pattern of services is at best a 'patchwork quilt' with very few comprehensive programs in place" (Forbush, 1978, p.92). Project Redirection was designed to provide and broker comprehensive programming, geared to the complex needs of disadvantaged adolescents who were parents. The program provided some services on-site, but many others were coordinated through linkages with other service providers in the community. Thus, the Project Redirection program in all sites included educational, employment-related, health, recreational, family planning, life management, and parenting components. Furthermore, unlike many other special programs that serve either pregnant teens or teen parents exclusively, Project Redirection offered continuity of service from pregnancy into the postpartum period.

An additional feature of the program model was its emphasis on teaching teens how to use services more effectively. Early efforts to address the problem of teenage parenthood were generally designed with adult patterns of service utilization in mind. The need for different models came to be recognized by service providers, policymakers, researchers, and advocacy groups alike in the 1970s and early 1980s (e.g. Washington, 1975; Cannon-Bonventre and Kahn, 1979; Schinke, 1978; Howard, 1971, 1978; Klerman, 1981; Klerman _et al._, 1983; Moore and Burt, 1982; Forbush, 1978). One of the characteristics of adolescents that observers noted was the teens' passivity in seeking assistance until a problem became a crisis. This slowness in obtaining services has been abundantly documented with respect to teenagers' tardiness in obtaining prenatal medical care (e.g. National Center for Health Statistics, 1982). These observations led several commentators to suggest that aggressive outreach and follow-up, as well as mechanisms to motivate teens to use services, would be essential to program success.

Project Redirection adopted three mechanisms designed to help teens take advantage of its services: the Individual Participant Plans (IPPs), community women, and peer group sessions. The IPP was a planning and monitoring tool that was developed jointly by program staff and the teen. The IPP specified the teen's short- and long-term goals and the services and activities designed to help her achieve them. As a monitoring document, the IPP showed on a monthly basis whether the teen had actually participated in scheduled activities.

The second mechanism was the involvement of "community women" in the program. These women were not professional caseworkers, but rather volunteers drawn from the local community to act as role models and supports to the teens. Assigned to the participants upon enrollment, the community women offered a non-institutionalized means of reinforcing the program's message and of helping the teens overcome the myriad personal difficulties these disadvantaged teens frequently encountered.

The third mechanism was peer group meetings, which brought participants together to discuss common and idiosyncratic problems and experiences. In addition to being a source of social support and mutual problem-solving, the peer group sessions provided staff with an additional opportunity to emphasize program goals.

Two other unusual features of the Redirection program as it was originally implemented deserve mention. First, program participants were paid a monthly stipend of $30.00. Second, the program at its inception had a linkage to a local Work Incentive Program (WIN). While teens of this age would normally be exempt from WIN, national officials were interested in promoting the employability of these teens in the long run. In two sites (Harlem and Phoenix), WIN stationed a local social service worker at the Redirection site. The WIN linkage was not continued in later phases of the demonstration.

The Project Redirection demonstration was broad in its scope, ethnic mix and enrollment. By the end of 1983, 1,000 Black, Hispanic and white teens had enrolled in the program in the four sites.

C. Project Redirection Implementation

The Manpower Demonstration Research Corporation (MDRC), a nonprofit corporation that focuses on policies relating to employment, training and social welfare, managed the implementation of Project Redirection and its program of related research. MDRC's operations staff monitored the local programs to ensure that the model was being implemented as designed. MDRC also maintained a detailed management information system on participants, community women, program activities, and program expenditures. Finally, MDRC instituted a comprehensive research program to document and evaluate the Project Redirection demonstration. The implementation experience has been fully described in two MDRC reports (Branch *et al.*, 1981, 1984), and is briefly summarized below.

The demonstration was implemented in three phases. During Phase I, which ran from June, 1980 to December, 1981, the program began operations in the four sites. The Phase I implementation report (Branch *et al.*, 1981) indicated that, although there were the inevitable start-up problems during the first few months of operation, the comprehensive program model proved to be operationally feasible. During this initial phase the community woman component was established and effective recruitment strategies were put in place. However, while most program components were operationalized during the first phase, some sites had problems in offering the full range of program services in a timely fashion. The most difficult services to provide or broker initially were employability training and educational services for the younger teens who were too young to enter such educational alternatives as GED programs. Implementation of the program was uneven across the sites, with the most serious difficulties encountered in Riverside. Details of the various operational problems of Phase I are fully described in Branch *et al.*, 1981.

During Phase II of the demonstration, which ran from January to December of 1982, MDRC provided more explicit service delivery guidelines that specified when activities were to commence for participants. The new guidelines required a minimum of 18 hours of employment-related activities for each participant. Additionally, the guidelines stipulated mandatory exit criteria: when participants turned 19, after 18 months of enrollment, or upon completion of high school or a GED program.

A recent report describing program operations during this second phase concluded that the local programs continued to coordinate comprehensive services to teens and that the employability component was considerably strengthened (Branch _et_ _al_., 1984). The report also described the local programs' success in working cooperatively with other community agencies in the provision of services. Nevertheless, program staff learned that, because of the distinctive characteristics of their clients--their extreme youth, their childrearing responsibilities, their academic deficiencies, and, for many, their multiple home-related problems--it was often easier and more effective to provide some services directly.

According to the Branch _et_ _al_. (1984) report, an important feature of the program during Phase II was the staff's attempts to reorient participants' values. The strategy generally used was to try to enhance self-esteem and to help teens view themselves as capable of redirecting their lives. High self-esteem came to be seen as a prerequisite to the commitment and motivation teens needed to attain long-range objectives.

The community woman component--perhaps the most innovative aspect of Project Redirection--generally worked well during Phase II (although the Riverside site experienced difficulties in recruiting an adequate number of women). Both staff and participants found the community women valuable. These women often provided useful information about the teens to program staff, allowing staff to make more well-informed judgments about the teens' activities and progress. The implementation of this component demonstrated the feasibility of using volunteers from low-income communities to assist other members of those communities.

Thus, it appears that by Phase II the program moved into a stable phase of operations that resulted in the direct provision or brokering of comprehensive services to a targeted group of teens. During Phase III, which ran from January to December of 1983, the demonstration continued in a transition mode in Harlem, Phoenix, and Riverside. The Boston program did not continue into Phase III (although a teen parent program was continued separately by the Boston agency). A major emphasis in this final phase was the identification of new funding sources to support the continuation of the program.

The Phase II implementation report also included an analysis of program costs. According to this analysis, the cost of maintaining one participant in the program for a full year was $3,893.

D. Project Redirection Research Plan

As described in the first section of this chapter, the Project Redirection program model incorporated several distinctive features into its operation. Another aspect of the demonstration that differentiated it from other teen parent programs was its rigorous and extensive research plan. The demonstration included an assessment of the implementation process (Branch, et al., 1981, 1984), an analysis of program costs (Branch et al., 1984); an in-depth ethnographic study (Levy, 1983); and an analysis of program impacts. The impact analysis portion of the research was conducted by Humanalysis, Inc. and the American Institutes for Research (AIR) under subcontract to MDRC. Data from the impact analysis study were previously published in a baseline report (Polit et al., 1982) and in an interim report (Polit et al., 1983). The baseline report presented basic descriptive information regarding a research sample of over 500 pregnant and parenting teens, prior to participation in Project Redirection. The interim report described program impacts 12 months after baseline. This report concludes the impact analysis of Project Redirection.

Certain features of this impact analysis deserve comment. The majority of existing programs for pregnant and parenting teens either have no evaluation component or use a design with so many methodological shortcomings that the results are difficult to interpret (see Klerman, 1979; Klerman *et al.*, 1983; Zellman, 1982). The Project Redirection impact analysis incorporated several distinctive design features, including the following:

- *Use of a comparison group* Redirection participants (the experimental group) were compared to a sample of comparable nonparticipants. The absence of a comparison group in teen parent program evaluation is common (e.g. Veerhusen *et al.*, 1972; Osofsky *et al.*, 1973; Smith *et al.*, 1971; Lansing, 1973; McAnarney *et al.*, 1978; Tatelbaum *et al.*, 1978; Edwards *et al.*, 1977; Youngs *et al.*, 1977; Cartoof, 1979; Burt *et al.*, 1984).

- *Large sample size* The final sample consisted of nearly 700 teenage mothers in the experimental and comparison groups combined. The vast majority of evaluations are based on samples of fewer than 100 teens. A notable exception is the recent evaluation of 26 programs funded by the Office of Adolescent Pregnancy Programs (OAPP) of the Department of Health and Human Services (Burt *et al.*, 1984), which represents the largest existing evaluation of teen parent programs. However, the data from this evaluation are difficult to interpret because of the absence of a comparison group.

- *Longitudinal design* Subjects were interviewed upon entry into the study and again 12 and 24 months later. Several other evaluations have collected follow-up data, but two-year follow-ups are not common.

- *Broad Scope* In the Project Redirection study a wealth of information was collected, both about background characteristics and about outcomes at 12 and 24 months after entry into the study. Efforts were made to mirror the comprehensive nature of the program in the scope of the interviews. Many evaluations focus on outcomes of a specific type, such as health-related outcomes (e.g. Osofsky *et al.*, 1973; Jorgensen, 1972; Youngs *et al.*, 1977; Smith *et al.*, 1978; Chanis, 1979).

- Statistical controls Statistical techniques were used extensively to estimate the effects of Project Redirection as precisely as possible. The Burt et al. (1984) evaluation of the OAPP projects is one of the few evaluations of a teen parent program that has employed rigorous statistical procedures.

These methodological strategies represented an attempt to determine as precisely as possible whether participation in an innovative program of services would result in improved outcomes in the areas of education, employability, family planning, and health. The design as implemented, however, proved problematic for assessing the impact of the program model on the target population. Two difficulties encountered during the course of the evaluation resulted in a design that yielded a conservative test of the impacts of Project Redirection.

First, since a randomized experimental design was not used, the possibility that the experimental and comparison groups were initially dissimilar was a source of concern. Data collected in the course of the study suggest that the comparison teens may have been more motivated than experimental teens. Consequently, the estimates of the program impacts may be less than its actual impacts. Selection biases are discussed in greater detail in Chapter 2.

Second, the comparison group was found to be a relatively well-served group. The majority had been in either school-based teen parent programs or other special programs designed to serve the target population. Nearly all comparison teens, whether they were in a special program or not, were getting many of the same services that Project Redirection offered. (See Chapter 3 for a complete description of service utilization by teens in this study). As a consequence, the impact analysis can best be construed as an evaluation comparing different treatment modalities. Comparison teens were generally receiving a wide range of services that existed in their communities, but perhaps in an uncoordinated fashion. The experimental teens received a similar array of services, but were exposed in addition to mechanisms designed to coordinate these services and to enhance their interaction (e.g. the community woman component). Thus, the impact analysis yielded information primarily about the incremental effectiveness of the coordinating mechanisms rather than about service provision per se. Therefore, given the conservative nature of the research design as implemented, this impact analysis is presumed to describe a lower bound of program effectiveness.

E. The Impact Analysis Report

The present report is the third and final report using data from the impact analysis survey. This report focuses primarily on impacts 24 months after entry into the study, but also includes material from the baseline and 12-month follow-up interviews.

Chapter 2 of this report describes the impact analysis design. Included in that chapter are a description of comparison group recruitment and sample selection; a discussion of response rates and attrition bias; an overview of measures and analytic approach; and an assessment of methodological limitations. Chapter 2 also describes the background characteristics of the study sample.

Chapter 3 examines the extent to which teens in the sample obtained various types of services. Given the potential discrepancies between what the research design hoped to accomplish and what it actually did accomplish, this discussion of service receipt is critical to the interpretation of the research findings. This chapter also examines some of the experiences of the Project Redirection participants, including length of enrollment for teens in various subgroups.

The next three chapters present the findings. Each of these chapters begins with a descriptive overview that shows how the subjects' lives have evolved over the 24-month period for which data were obtained. Then the results of the impact analysis are presented. Chapter 4 focuses on fertility-related outcomes such as subsequent pregnancies and contraceptive use. Chapter 5 examines data relating to educational outcomes. The sixth chapter is concerned with employment, employability and welfare dependency.

The concluding chapter (Chapter 7) highlights the major findings of the analytic work. The final chapter also interprets the results and discusses the implications of the findings with regard to public policy and service provision for teenage parents.

The appendices present primarily supplementary technical information. Appendix A describes some methodological concerns relating to the recruitment of subjects at two different points in time. Appendix B discusses analytic strategies. Appendix C presents analyses relating to sample attrition. Appendix D consists of information comparing the results of this study with findings from other research on teen parents. Supplementary statistical tables are included in Appendix E. Bibliographic references are listed in Appendix F. Finally, a glossary of terms frequently used in this report is included in Appendix G.

CHAPTER 2

THE IMPACT ANALYSIS RESEARCH DESIGN

A. Overview of the Design

During an initial planning period, the possibility of using a randomized experimental design to assess the impacts of Project Redirection was explored and ultimately considered unfeasible. The major obstacle to implementing a true experiment was the projected size of the applicant pool. The program's early recruitment experience suggests that initial projections were reasonably accurate. The low number of applicants at the outset made it impossible for programs to meet their initial quotas, even with all referred teens becoming participants.

In the absence of a randomly assigned control group, the most appropriate design for an assessment of program impacts involves the selection of a comparison group as similar as possible to the experimental group prior to program exposure. For this study, the comparison group consisted of teens meeting the program's eligibility criteria in cities not offering the Redirection program, but matched to the Redirection sites on various socio-economic and geographic indicators. The matched experimental/comparison site pairs were as follows: Boston, Massachusetts—Hartford, Connecticut; Harlem, New York—Bedford Stuyvesant, New York; Phoenix, Arizona—San Antonio, Texas; and Riverside, California—Fresno, California.

B. Samples I and II

The original research design called for interviews with experimental and comparison group respondents at baseline (within one month of program enrollment for experimental teens), 12 months after baseline, and 24 months after baseline. This design was implemented for those teens who enrolled in Project Redirection from the time it began operations (August 1980) to the spring of 1981. These teens, together with their comparison group counterparts, comprise Sample I. Sample I at baseline consisted of approximately 450 teens.

In the fall of 1981, a decision was made to augment the research sample by adding teens who had enrolled after March 1981. The decision was based on an interest in examining program impacts for certain key subgroups of teens, which necessitated a larger sample. The new sample (referred to as Sample II) included teens who enrolled in Project Redirection between the months of March 1981, and January 1982, together with their comparison group counterparts. Because the decision to add Sample II to the research design was made after teens enrolled in the program, there was no opportunity to administer a baseline interview. Thus, initial interviews with Sample II experimental teens were conducted 12 months after enrollment. For Sample II comparison teens, the initial ("12-month") interview was administered at an arbitrary date, with selection criteria specifying that the comparison teens should have been eligible for inclusion in the study 12 months earlier. Thus, in Sample II the date of "baseline" was established as 12 months before the first interview for comparison teens and as the date of enrollment for experimental teens. Sample II teens were re-interviewed at 24 months post-baseline.

During the initial (12-month) interview with Sample II respondents, detailed school, work, and pregnancy histories were obtained. These histories provided information about the baseline characteristics of these respondents, and this information was subsequently used in developing statistical controls. The reliability of these retrospective baseline variables is discussed in Appendix A. Thus, although only two interviews were conducted with Sample II teens, the report refers to characteristics of these teens at baseline, 12 months post-baseline, and 24 months post-baseline. Sample II consisted of approximately 350 teens in the initial (12-month) interview.

Sample II was added to the impact analysis research design to strengthen the sensitivity of the analyses and to make possible the examination of subgroup impacts, including impacts at the site level. Sample I alone was too small to permit a scrutiny of site-level effects. However, given the unusual nature of the Sample II design, the question of whether the two samples could be aggregated needed to be addressed. Analyses concerning the appropriateness of aggregating the two samples were conducted, focusing primarily on the following three issues: (1) the absence of data obtained at baseline for Sample II teens; (2) the comparability of the Sample II experimental and comparison groups; and (3) the comparability of Samples I and II. The results of these analyses, which are fully described in Appendix A, indicated that data from Samples I and II could be pooled for almost all analyses. The exceptions involved situations in which it was impossible to obtain retrospectively a baseline variable from Sample II teens that would serve as a key control variable in an impact analysis (e.g. baseline scores on a Birth Control Knowledge

Test when analyzing program impacts on birth control knowledge). Thus, most of this report presents information for the aggregated sample.

C. Implementation of the Research Design

Given the quasi-experimental nature of the research design, a major concern was the potential nonequivalence of the experimental and comparison groups at baseline. Such nonequivalence--referred to as selection bias--could result in biased estimates of program impacts. If experimental group teens would have had more favorable outcomes than comparison group teens even without participating in Project Redirection, then a positive selection bias would exist and would make the program appear more effective than it actually was. If, on the other hand, comparison group teens would have done better than experimental teens in the absence of the program, a negative selection bias would mask some of the positive effects the program did have.

There are no methodological strategies that can completely eliminate the risk of selection bias in a non-randomized design. There are, however, several steps that can be taken to minimize its threat. Given the limitations of the basic design, this study used a variety of methodological techniques to reduce selection bias. Strategies relating to sample selection and data collection are discussed below, while analytic techniques are presented in another section.

In selecting comparison group members, special attention was paid to issues bearing on the selectivity bias problem. Among the steps that were taken are the following:

- Eligibility. Only young women who met program eligibility criteria were included in the comparison sample. (For Sample II, comparison group teens were eligible only if, one year prior to their first interview, they would have met program eligibility criteria). This means that the teens in both the experimental and comparison groups were young, poor, had not completed their basic schooling, and were either pregnant or a parent. The net result was a research sample that was homogeneous with respect to many variables known to be related to educational attainment, employment, and lifetime fertility

- Site Selection. Comparison sites were matched to experimental sites in terms of geographic region, teen pregnancy rate, ethnic distribution, per capita income, population density, welfare rate, poverty rate, and service availability for teen parents. The baseline report (Polit *et al.*, 1982) presents information on the degree of match between paired sites. The use of communities other than the experimental sites themselves guaranteed a sample that would not be composed entirely or predominantly of refusers (i.e. those who selected themselves *out* of the program).

- Recruitment. Teens in the comparison sites were recruited into the research sample in a manner analogous to the recruitment of experimental group teens into the Redirection program. The principal means of recruitment was through referral from community agencies (hospitals, schools, social service agencies) and word-of-mouth referral from teens already in the sample.

- Matching. Although pair-matching of individual teens was not considered a viable option because of constraints on the pool of eligible teens in each site, efforts were made to "balance" the experimental and comparison groups on four dimensions: age, ethnicity, baseline parity, and receipt of services from teen parent programs. In other words, similar proportions of teens in different age, ethnic, parity and service receipt groups were recruited for each pair of matching experimental/comparison sites. While perfect matching was not possible because of a restricted pool of eligible teens, the balancing procedure prevented any extreme dissimilarities of the two groups on these four important dimensions.

While the implementation of these techniques was relatively straightforward for Sample I, these design features were more problematic for the selection of Sample II comparison teens. The recruitment of Sample II comparison teens was accomplished using the same techniques as for Sample I--primarily through the assistance of service providers who were in contact with the target population. For both samples, this procedure resulted in a comparison group of teens who were connected with the social services delivery system, and who, therefore, should not be construed as a "no treatment" group. Given that Redirection participants were also recruited through service providers and were also connected with a network of service delivery, the comparison group strategy seemed justifiable. However, for Sample II, the comparison group consisted predominantly of teens who were either still in contact with service providers one year after they should have been baselined or who were at least easy to locate using agency records. This suggests that Sample II comparison teens might be either especially well linked with a service delivery network or living in a fairly stable home environment, or both. In other words, the possibility of selection biases favoring motivated and well-served comparison teens appears to be greater in Sample II than in Sample I.

The problem of recruiting teens who would have met program eligibility criteria a year earlier led to yet another problem for the Sample II comparison group. It proved to be more difficult to recruit comparison teens who had already delivered their infants one year prior to the first interview than to recruit teens who were pregnant a year earlier. This is understandable, given the high level of service use during pregnancy and the immediate postpartum period. In other words, service providers who made referrals to the study were more likely to be in touch with teens who recently delivered their babies than with teens whose babies were a year old or more. Despite the fact that baseline parity was one of the four matching criteria, this match proved difficult to implement in Sample II. Consequently, 59% of the experimental teens, but 70% of the comparison teens in Sample II were pregnant at baseline. Furthermore, since "baseline" for comparison group teens was defined as 12 months before the initial interview, but "baseline" for experimental teens was defined as date of enrollment, there was a somewhat longer interval between baseline and the 12-month interview for experimental teens (mean = 14.2 months) than for comparison teens (mean = 12.0 months). The result of these two problems was that experimental teens in Sample II were at risk of a repeat pregnancy for a substantially longer period of time than comparison teens. Between the delivery of the index pregnancy (the pregnancy nearest to baseline) and the date of the 24-month interview, the mean number of days elapsed was 794 for experimental group members and 659 for comparison group members in Sample II.

Despite the efforts outlined earlier to develop comparable groups of experimental and comparison teens, analysis of baseline data revealed that the two groups did differ initially on several traits in both samples. Table 2.1 summarizes a number of important baseline variables for experimental and comparison teens in both samples who were re-interviewed at 24 months post-baseline.[1] The most noteworthy difference was that comparison teens were substantially more likely than experimental teens to be in school at baseline. In both Samples I and II, nearly two-thirds of the comparison teens were enrolled in school at baseline, compared with fewer than half of the experimental teens. Other group differences were restricted to either Sample I or II. Experimental teens in Sample I had spent less time in special school programs at baseline than comparison teens and came from smaller families. In Sample II, experimental teens had dropped out of school more frequently, were younger when they first gave birth, were less likely to be married, and had more time at risk to a subsequent pregnancy than comparison teens.

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[1] The variables included in Table 2.1 were used as covariates in the impact analyses, as shown in Appendix E. The statistical tests summarized in Table 2.1 and subsequent tables are two-tailed tests.

TABLE 2.1

COMPARISON OF EXPERIMENTAL AND COMPARISON GROUP MEMBERS ON SELECTED BASELINE VARIABLES, BY SAMPLE

Variable	Percentages or Means, by Group and Sample			
	Sample I		Sample II	
	Experimental	Comparison	Experimental	Comparison
• Demographic				
Mean Age	16.3	16.5	16.4	16.5
Percent Married	3.8	8.2	2.3*	10.1
Percent Pregnant, not a Parent	53.8	58.0	59.2+	70.4
Percent Black	48.1	43.0	49.2	45.9
Percent Mexican American	23.1	30.0	21.5	21.4
Percent Puerto Rican	12.2	14.5	19.2	25.2
Percent White	12.2	10.1	5.4	6.9
• Educational				
Percent in School/GED Program	46.2***	65.7	45.4**	62.3
Mean Highest Grade Completed	8.7	8.9	8.9	8.9
Mean Number of Times Dropped out of School	.71	.70	.74**	.50
Mean Number of Semesters in a Teen Parent Program	.38***	.68	.43	.45
Percent in a Teen Parent School Program at Baseline	22.9	29.9	16.2	15.7
• Family Planning/Fertility				
Mean Number of Pregnancies	1.2	1.3	1.2	1.2
Percent With One or More Abortions	7.1	6.3	1.5	4.4
Mean Age at First Birth	16.1	15.9	15.8**	16.5
Percent Ever Used Birth Control	45.5	51.7	40.8	34.0
Percent Used Oral Contraceptives	34.9	35.1	21.5	18.2
Mean Score on Birth Control Knowledge Test	9.1	9.3	--	--
• Employment				
Percent Employed	10.3	10.1	6.9	11.9
Percent Ever Worked	71.8	69.6	49.2	53.5
Mean Number of Jobs Held	1.3	1.1	0.7	0.8

(continued)

TABLE 2.1 (continued)

Variable	Sample I		Sample II	
	Experimental	Comparison	Experimental	Comparison
• Home Environment				
Percent in AFDC Household	74.4	73.4	56.9	50.9
Percent Raised by Both Parents	25.1	25.1	30.0	37.1
Percent Whose Mother Was a Teen Mother	70.5	72.9	62.3	59.7
Mean Number of Siblings	4.8**	5.7	4.6	4.7
Percent Whose Mothers Completed High School/GED	32.0	25.6	30.0	29.6
Percent Whose Fathers Com- Completed High School/GED	23.4	27.0	24.6	26.4
• Other				
Mean Number of Days Between Date of Baseline and Date Index Pregnancy[a] Terminated	30.4	27.4	3.9*	-46.5
Mean Number of Months Between Date of Baseline and Date of 24-Month Interview	27.7	26.2	26.3***	23.5
Number of Respondents	175	211	130	159

SOURCE: Tabulations are from AIR interviews with experimental and comparson group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: Tests of statistical significance (chi-square test for percentages and two-tailed t-tests for means) were performed between experimental and comparison groups within samples. Significant differences are indicated next to the experimental percentages or means.

[a] The index pregnancy was either the pregnancy in progress or the most recently terminated pregnancy at baseline.

+The group difference is statistically significant at the .10 level.

*The group difference is statistically significant at the .05 level.

**The group difference is statistically significant at the .01 level.

***The group difference is statistically significant at the .001 level.

However, the two groups in both samples were similar on a large number of important background variables. The groups were similar in age, ethnic composition, highest grade completed, contraceptive experience, number of pregnancies, employment experience, AFDC receipt, and parental background. Taken in the aggregate, the experimental and comparison group differences were relatively few in number. Nevertheless, it is clear that the two groups were not, in fact, equivalent. The most disturbing differences were the baseline school status and the length of time at risk of a repeat pregnancy. Both of these variables are important determinants of outcomes such as school completion and subsequent fertility. The need for statistical controls to remove such differences is clear, and analytic strategies are described in the next section.

D. Basic Analytic Strategies

Selection biases can generally be reduced through statistical procedures. In the present study, several multivariate techniques were adopted to remove or reduce initial group differences, as well as to improve the precision of the estimates of impacts. These techniques are summarized briefly below and are discussed in more detail in Appendix B.

The basic approach was to use analysis of covariance (ANCOVA), which is the most widely used analytic technique in quasi-experimental designs. ANCOVA is used to statistically adjust estimates of treatment effects for measured differences in the pre-treatment characteristics of the experimental and comparison groups. To the extent that selection bias is associated with the statistically controlled characteristics (covariates), the selectivity problem will be reduced and possibly eliminated.

Several considerations, however, called for a departure from the basic ANCOVA approach in certain situations. For example, the use of the linear model may not be warranted when the outcome of interest is a binary (dichotomous) variable (e.g. school completion). A nonlinear maximum likelihood estimation technique, logit analysis, was generally used to estimate treatment effects when the outcome variable was dichotomous. Additionally, special two-stage regression analyses were performed in an effort to correct selection biases. These supplementary analyses are described in Appendix B.

In addition to using several alternative analytic techniques, treatment effects were also examined in several ways. The basic technique was to create a dichotomous variable indicating membership in either the expermental or comparison group. The results chapters focus primarily on program impacts as estimated by this dichotomous comparative procedure.

Two other procedures were used to explore more specific aspects of program participation. First, the dichotomous experimental/comparison variable was replaced with a variable indicating amount of program exposure (i.e. length of time enrolled in the program). Second, in some analyses, receipt of specific Redirection services (e.g. educational counseling) was substituted for program enrollment. These two supplementary techniques were designed to shed some light on whether increased exposure to Project Redirection, or the receipt of specific services from it, was associated with improved outcomes.

Unfortunately, while the logic of using alternative treatment measures is straightforward, the interpretation of results from such analyses is not. The problem stems from the fact that the alternative treatment variables may be confounded with subject characteristics. When length of enrollment, for example, is used as the treatment variable, this may be measuring both intensity of program inputs and client characteristics (such as perseverance or motivation) that lead teens to take advantage of the program's services. In other words, self-selection affects length of program stay and receipt of specific services. If length of stay is found to have a positive impact on outcomes, there is no way of knowing definitively if teens who remained a long time would have done as well even with shorter enrollments because they were more motivated, more aggressive, more competent, and so on. A similar argument could be made in the case of receipt of specific services.

While readers are urged to exercise extreme caution in drawing conclusions about the effects of service intensity or receipt of specific services on outcomes because of this self-selection issue, the results of these analyses merit consideration for several reasons. First, it is plausible to expect that longer involvement in a program will yield better outcomes than short-term enrollment; thus, despite the confounding selection problem, it is a hypothesis that deserves examination. Second, extensive statistical controls were employed in the alternative treatment analyses, so that many of the characteristics that would lead a teen to take advantage of services were held constant. Third, a major concern in the analyses using the simple experimental/comparison contrast was that comparison teens may have been more motivated at baseline than experimental teens. Therefore, it seemed plausible that contrasting comparison teens with long-term Project Redirection teens might actually result in groups with greater initial equivalence. An inspection of some baseline characteristics supports this hypothesis. Some comparative means and percentages for short-term and long-term enrollees and comparison teens on baseline characteristics is summarized below:

	Comparison Teens	Experimentals < 12 Months	Experimentals 12 Months or More
In School/GED Program	64%	44%	48%
In a Teen Parent Program	24%	18%	23%
Raised by Both Parents	25%	19%	24%
Ever Held a Job	62%	56%	69%
Number of Times Dropped out of School	.61	.79	.63
Number of Semesters in a Teen Parent Program	.49	.34	.43

Thus, although we emphasize that it is inappropriate to infer a causal link between length of exposure or receipt of specific services on the one hand and outcome behaviors on the other, the results of these alternative analyses are at least suggestive and merit some consideration in the overall assessment of Project Redirection.

One other aspect of the analytic strategy should be described, and that is the analysis of program impacts for selected subgroups of teens. In addition to examining experimental and comparison group differences for the aggregated sample, separate impact analyses were performed for several subgroups of special interest. The dimensions used in disaggregating respondents were as follows:

- Ethnicity (Black, Mexican American, and Puerto Rican; there were too few whites to perform a separate subgroup analyses);
- Age (Teens age 15 or younger at baseline vs. teens age 16 or 17 at baseline);

- Pregnancy Status (Teens pregnant at baseline vs. teens who had already delivered at baseline);

- Welfare Status (Teens in a household with one or more AFDC recipient at baseline vs. teens not in an AFDC household);

- School Status (Teens in school at baseline vs. teens not in school at baseline);

- Work History (Teens with employment experience at baseline vs. teens without employment experience at baseline);

- Site (Boston vs. Hartford; Harlem vs. Bedford Stuyvesant; Phoenix vs. San Antonio; and Riverside vs. Fresno); and

- Sample (Sample I vs. Sample II).

The major intent of these analyses was to examine the pervasiveness of any program impacts and to determine whether the program worked especially well for certain groups of teens. The results of these subgroup analyses were expected to be useful in targeting Redirection-type programs to those who might benefit most from them.

Two cautionary notes regarding the subgroup analyses should be added, and both relate to site-specific data. First, site and ethnicity are seriously confounded. For example, virtualy all the Puerto Rican teens were from Boston and Hartford; nearly all the Harlem and Bedford-Stuyvesant teens were black. Table 2.2 presents a breakdown of the final research sample by site and ethnicity.

The second is that the results from matched-site comparisons should be treated cautiously. While in the aggregate the experimental and comparison groups were fairly similar initially, as shown in Table 2.1, there were more notable differences at the site level. Furthermore, events and circumstances occurring within sites during the study period could obviously have a greater effect on site outcomes than on aggregated outcomes. For example, the economic recession of the early 1980s was more

TABLE 2.2

ETHNIC COMPOSITION OF FINAL IMPACT ANALYSIS SAMPLE, BY SITE

Site	Percentage Distribution of Teens, by Ethnicity					
	Black	Mexican American	Puerto Rican	White	Other	All Ethnic Groups
Boston	0.0	0.0	95.6	0.0	4.4	100.0 (N= 45)
Harlen	91.5	0.0	3.7	0.0	4.9	100.0 (N= 82)
Phoenix	45.0	42.1	0.7	8.6	3.6	100.0 (N=140)
Riverside	28.9	23.7	0.0	39.5	7.9	100.0 (N= 38)
Number in Experimental Sites	149	68	47	27	14	305
Hartford	0.0	0.0	97.1	0.0	2.9	100.0 (N= 69)
Bedford-Stuyvesant	93.3	0.0	4.8	0.0	1.9	100.0 (N=104)
San Antonio	37.3	55.3	0.0	7.3	0.0	100.0 (N=150)
Fresno	23.4	27.7	0.0	44.7	4.3	100.0 (N= 47)
Number in Comparison Sites	164	96	72	32	6	370
Total Number of Respondents	313	164	119	59	20	675

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at the 24-month interview.

severe in some sites than in others. As a further example, the availability of services changed dramatically in some cities (e.g. services to teen parents in Fresno were greatly expanded). Thus, while site-level comparisons are of interest, their results must be interpreted with these considerations in mind.

In summary, a variety of analyses were conducted to evaluate the effectiveness of participation in Project Redirection. The fundamental approach was to use a dummy treatment variable (experimental versus comparison group) in an ANCOVA model, using pre-treatment characteristics (including the baseline status on the outcome variable) as covariates. Additional analyses were performed to (1) deal with situations in which assumptions of the ANCOVA model were likely to be violated; (2) further explore handling selection biases; (3) refine the measurement of the treatment variable; and (4) examine program impacts for selected subgroups of teens.

E. Sample Size and Completion Rates

In the experimental sites, the Sample I fielding strategy was to conduct baseline interviews with all participants who enrolled in Project Redirection between September 1980 and August 1981 and remained in the program for at least 30 days. However, for budget purposes, a sampling quota was established at each site based on the number of interviews (and hence sample sizes) needed for the analysis. Interviewing stopped when those quotas were reached. (For a more detailed description of the field work, see Kahn et al., 1984.)

Overall, 90% of enrollees were "fielded": that is, attempts were made to contact and interview almost all of the teens. The 10% not contacted had either dropped out of the program within 30 days of enrollment or had enrolled after the maximum number of interviews had been obtained. Of the teens fielded, 80% were administered a baseline interview. (The one-fifth not interviewed either could not be located or were known to have moved from the area.) None of the experimental teens contacted refused to be interviewed, so that the resulting 449 cases with a baseline history formed the complete sample.

In the comparison sites, community agencies generally referred willing and eligible teens to interviewers, who then screened prospective respondents to ensure they were appropriate in terms of matching criteria. A few teens declined to be interviewed at the last minute, but the exact refusal rate in not known.

Completion rates for Sample I at 12 months and 24 months post-baseline are shown for each site in the top half of Table 2.3. The overall completion rate for Sample I was 89% at 12 months and 86% at 24 months. However, as this table indicates, the completion rate was considerably lower in the experimental sites (79% at 24 months) than in the comparison sites (93% at 24 months).

The survey operation was more difficult in the experimental sites for Sample II than for Sample I, and consequently response rates were lower. With Sample I, virtually all experimental teens were still enrolled in the program at the time of the baseline interview, which was scheduled for completion within 45 days of enrollment. This meant that the program staff or community women could provide interviewers with good contact information, as well as information about the teens' schedules. In some cases, when teens were particularly difficult to contact, interviews were scheduled at the program site to coincide with planned activities for the

TABLE 2.3

SAMPLE SIZES AND RESPONSE RATES FOR THE IMPACT ANALYSIS STUDY AT BASELINE, 12-MONTH, AND 24-MONTH INTERVIEWS, BY SITE AND SAMPLE

A. SAMPLE I

Group and Site	Number Interviewed at Baseline	Number Interviewed at 12 Months	Number Interviewed at 24 Months	Final Response Rate[a]
Experimental Sites:				
Boston	36	27	24	66.7%
Harlem	56	40	38	67.9%
Phoenix	89	82	81	91.0%
Riverside	42	36	32	76.2%
TOTAL	223	185	175	78.5%
Comparison Sites:				
Hartford	35	31	33	94.3%
Bedford-Stuyvesant	62	57	54	87.1%
San Antonio	89	88	86	96.6%
Fresno	40	39	38	95.0%
TOTAL	226	215	211	93.4%
TOTAL SAMPLE I	449	400	386	85.7%

B. SAMPLE II

Group and Site	Number Interviewed at Baseline	Number Interviewed at 12 Months	Number Interviewed at 24 Months	Final Response Rate
Experimental Sites:				
Boston	(58)[b]	36	21	58.3%
Harlem	(86)	59	44	74.6%
Phoenix	(81)	66	59	89.4%
Riverside	(11)	9	6	66.7%
TOTAL	(236)	170	130	76.5%
Comparison Sites:				
Hartford	—	39	36	92.3%
Bedford-Stuyesant	—	56	50	89.3%
San Antonio	—	66	64	97.0%
Fresno	—	9	9	100.0%
TOTAL		170	159	93.5%
TOTAL SAMPLE II		340	289	85.0%

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] The final response rate is the percent of teens initially interviewed (at baseline for Sample I and at 12-months for Sample II) who completed a 24-month interview.

[b] The numbers in parentheses are the participants who enrolled in Project Redirection between March, 1981 and January, 1982 and who, therefore, were eligible to be in Sample II. Many of these teens could not, however, be found for the initial (12-month) interview. Teens never interviewed were not included in the denominator when calculating final response rates.

teens. With Sample II, the majority of teens were no longer enrolled in the program at the time of the initial (12 months post-baseline) interview. Contact information tended to be incomplete or inaccurate. The result was that 12-month interviews with experimental teens were completed with 72% of those cases that were fielded to the interviewers. Of the teens never interviewed, 86% were teens who could not be located; only 11% were refusals. Sample II comparison group members, by contrast, were interviewed on an as-referred basis, as was the case for Sample I comparison group respondents; consequently the completion rate for Sample II comparison teens was 100% at the 12-month interview.

The bottom portion of Table 2.3 shows final response rates for the eight sites in Sample II. In the experimental sites, 130 (77%) of those initially interviewed were re-interviewed at 12 months later, a rate similar to the final completion rate for Sample I experimental teens. However, these 130 experimental teens constituted only 55% of all the teens who enrolled in the program between March 1981 and January 1982 and were targeted for an interview. The final completion rate for Sample II comparison teens was 94%.

In order to provide some perspective to the issue of attrition in the present study, comparative data were sought in other studies. Excluding Sample II teens, whose 24-month completion rates do not reflect two-years of contact with the study team, the final completion rates in this study are similar to or better than the completion rates in other studies of teenage mothers at about 24 months after a baseline date, as shown below:

●	Project Redirection, experimental teens (Sample I)	79%
●	Project Redirection, comparison teens (Sample I)	93%
●	Burt *et al.*, 1984—OAPP-project participants	69%
●	Klerman and Jekel, 1973—Young Mothers Program participants	82%
●	Flick, 1983—Parent-Infant Interaction Program participants	38%

Flick, 1983--unserved comparison teens	25%
McAnarney *et al.*, 1978--Rochester Adolescent Maternity Project participants	56%
McAnarney *et al.*, 1978--community health center clients	58%
McAnarney *et al.*, 1978--hospital obstetric clinic clients	46%

The striking figure in this list, in fact, is the high completion rate of the comparison group teens in the present study. Several factors probably contributed to the low comparison group attrition. First, this group by definition consisted of teens who volunteered to be interviewed. Second, teens in the comparison group were offered an incentive payment of $10 for each interview, while experimental teens were not. It was originally predicted that, without a subject stipend, it would be difficult to recruit and maintain a comparison sample. Third, more vigorous efforts were made to stay in touch with the comparison group than the experimental group during the inter-interview periods. (The rationale was that, in the experimental group, program staff and community women could be called upon to supplement information about a teen's whereabouts in the event of a move.) For comparison group members, follow-up postcards were sent every three months. Respondents were asked to indicate their current address and a prospective new address, if known and applicable. As an incentive for teens to return their postcards, each return was treated as any entry into a lottery. At the completion of the follow-up interviews, the postcards were drawn at random and gift certificates for up to $250 were awarded to the winners in each site. It appears that the combination of these strategies resulted in a very high completion rate in the comparison sites.

The higher rate of attrition in the experimental than in the comparison groups, like the selectivity issue, poses a potential threat to the internal validity of the study. If teens with favorable outcomes tended to select themselves out of the follow-up surveys, then a negative attrition bias would arise, making the program appear to be less effective than it really was. This might be the case if, for example, non-completers were more likely to hold full-time jobs and had too little time for the interview. On the other hand, if teens with unfavorable outcomes tended not to be re-interviewed, then a positive attrition bias would result, making the program appear more effective than it really was.

Several steps were taken to address the issue of attrition biases. First, teens who completed the 24-month interview were compared with teens who did not in terms of characteristics measured during the initial interview (the baseline interview for Sample I and the 12-month interview for Sample II). Overall, few significant differences were observed. In Sample I, the 386 teens re-interviewed at 24 months were significantly more likely to be living with their mothers at baseline, likely to have completed more schooling, less likely to be enrolled in a teen parent school program, and more likely to already have a baby at baseline than the 63 teens who were not re-interviewed. In Sample II, the 287 teens who completed the 24-month interview were significantly more likely to have been living with their mothers at the initial interview and to have been raised in a household with both parents present than the 50 teens who were not re-interviewed. In both samples, the completers and non-completers were similar with respect to age, marital status, number of siblings, ethnicity, employment history, AFDC receipt, pregnancy history, and service utilization (The full tables are presented in Appendix C.)

The pattern of differences between the initial and follow-up samples suggest a slight tendency for teens from less stable backgrounds to have dropped out of the study. In both Samples I and II, teens who were not living at home were less likely to be reinterviewed. This may reflect greater disadvantage, in the sense that the teen would not have a built-in support system. On the other hand, it could reflect greater independence on the part of these teens. Aside from the socio-psychological interpretations that could be attributed to this variable, the fact is that teens who had moved from home were simply more difficult to track in the follow-up period. Overall, the small differences do not paint a picture of striking disparities between completers and noncompleters. In the important areas of employment, schooling, and pregnancy history at baseline, attrition biases seem fairly small, at least with respect to the variables measured.

With Sample II, a second issue was the extent to which the 167 experimental teens interviewed initially differed from the 82 program participants who were never interviewed. Using data from MDRC's management information system, it was possible to compare these two groups in terms of characteristics recorded on the enrollment form. These data suggest that those never interviewed were more disadvantaged than those in the research sample. For example, those teens never interviewed were, at enrollment, more likely to be heads of household, more likely to speak limited English, less likely to live with both parents, and less likely to be getting medical care for their children. On the other hand, in terms of program participation, those never interviewed received more medical and educational services. It should also be noted that the two groups were not significantly different with respect to such variables as school status at enrollment, highest grade completed, AFDC status, and number of household members.

Unfortunately, nothing further can be done to analytically correct for differences between those in and not in the research sample. However, there are analytic procedures to address the attrition problem for those teens who were initially in the research sample but who were not re-interviewed. The procedure is a two-stage statistical technique recently developed by economists. In the first step, baseline variables are used to model the attrition process. Then the results of the first step are used to correct for attrition in the estimates of program impact. The results of these analyses, described in Appendix C, indicated that attrition could not be accurately modeled and that correction factors, as a consequence, had no effect on program impacts. In other words, although experimental teens were less likely to be re-interviewed than comparison teens and although completers and noncompleters were different with respect to several variables, systematic differences appeared to be too small to change the nature or magnitude of program impacts.

F. Methodological Assessment

In order to provide a context for interpreting the results presented in the chapters that follow, this section reviews some of the potential biases that could have affected those results. Given the quasi-experimental nature of the design, there are several possible sources of bias to be considered.

The major potential biases in studies using the present design are referred to as maturation, history, self-selection and attrition biases (Campbell and Stanley, 1963). Maturation refers to changes resulting from the passage of time or from related developmental processes. Since the experimental and comparison group were matched for both age and parity (and thus roughly for age of the children), there seems little reason to suspect that the results were affected by differential maturation in the two groups.

The bias known as "history" refers to the occurrence of events external to but concurrent with the experimental treatment that could affect the outcomes of interest. This problem is one that could emerge even in a randomized experiment. For example, an alternative teen parent program could open in which control group members could enroll. Given the length of the study period, it is likely that there were many forces that affected the final outcomes. The declining economy is one such force that was observed to affect employment-related behaviors. However, the important question is whether there were events or circumstances occurring during the study period that systematically affected the comparison and experimental teens differently. The declining economy presumably affected teens in the two groups similarly.

One possibility that merits attention is that services aimed at the target population became increasingly available during the course of the study in the comparison sites. This did, in fact, appear to occur. Initially, the experimental and comparison sites were matched in terms of the array of services available to pregnant and parenting teens. Furthermore, one of the criteria on which comparison teens were "matched" to experimental teens was prior experience in a special program. However, the 1980 to 1983 period witnessed an escalation of public concern about the problems of teenage parents and the initiation or expansion of special services for them. An informal review of the service delivery network in comparison sites in the spring of 1984 suggested that many teen parent programs began or expanded operations in the early 1980s. In Fresno, for example, a program started that was modeled after Project Redirection.[2] Data collected for this study also revealed that over half of the comparison teens had been clients in a special teen parent program themselves. These factors could result in a dilution of measured program effects.

The third potential bias in this study is self-selection bias. This bias concerns the possibility that the teens in the two groups were initially nonequivalent, and that pre-existing differences affected the observed outcomes. As indicated above, the two groups were, in fact, known to be different initially. Generally, comparison teens seemed to be more motivated than experimental teens. On the basis of findings from other studies, the characteristics that distinguished the two groups would be expected to be associated with improved final outcomes for comparison teens.[3] The likelihood that comparison teens were more motivated than experimental teens initially is strengthened by the knowledge that, particularly for Sample II, teens recruited into the comparison group had well-established links to service providers in their communities. All of these factors would be expected to lead to reduced program impacts.

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[2] The informal survey of service providers suggested that Fresno had the best array of services for pregnant and parenting teens of any of the eight sites at the end of the survey, and that coordination among services was high. Available services included the Fresno County program "Resources and Education of Adolescent Parents"; hospital programs; two satellite school programs; a program that included community women offered by the Children's Home Society; and a program coordinated through the juvenile courts.

[3] Data from the present study confirm that these variables are important determinants of educational, employment, and fertility outcomes. For example, as shown in the regression tables in Appendix E, teens in school at baseline were more likely to delay a subsequent pregnancy, to spend several semesters in school post-baseline, and to have completed school than teens who were dropouts at baseline. Teens who were pregnant at baseline were less likely to have a subsequent pregnancy during the follow-up period and teens who had their first child while they were very young were more likely to have a subsequent pregnancy, less likely to spend several semesters in school, and less likely to have held several jobs than teens who delayed their first birth.

Selection biases of a different nature may exist due to the Sample II program participants who were never interviewed. These teens generally came from somewhat less stable backgrounds than teens who were study subjects (e.g. they were less likely to be living with both parents). However, had they been included, even wider disparities might have occurred in the baseline characteristics of experimental and comparison teens. In other words, given the comparison teens' initial advantage relative to Redirection participants who were in the research sample, the two groups were probably more equivalent at baseline because of the exclusion of these 80 teens from the experimental group.

The fourth potential bias in this study is selective attrition. Since experimental teens had a substantially higher rate of attrition than comparison teens, the possibility exists that outcomes would reflect differences in the characteristics of teens remaining in or exiting from the study. However, in the multivariate analyses described in Appendix C, the only variables significantly distinguishing noncompleters and completers after group and site variables were controlled were ethnicity and pregnancy status. Teens who were pregnant at baseline were less likely to complete the study than teens who entered as mothers. Since most noncompleters were experimental teens, this tendency for pregnant teens to disproportionately leave the study exacerbated the different lengths of time at risk to a repeat pregnancy among experimental and comparison teens. Thus, selective attrition could have resulted in either positive or negative biases, but the net affect appears to be negligible.

One additional piece of information relating to selective attrition concerns teens who left the study between the 12- and 24-month interviews. At the 12-month interview there was a total of 737 teens, while at the final interview there were only 675 teens. There is some evidence that those who left the study between the two follow-up interviews might be teens who were affected more positively by the program than those who remained. For example, the experimental teens did substantially better than comparison teens on a key outcome (school status 12 months after baseline) when the full 12-month sample of 737 cases was used. The 12-month differences were markedly reduced when the same analysis was performed with the 675 cases available in the final research sample.[4] Because attrition was primarily observed in the experimental group, this reduction suggests that a disproportionate percent of those positively affected by Project Redirection were not re-interviewed at 24-months post-baseline.

Table 2.4 summarizes the various potential sources of bias in the impact analysis. In the aggregate, the various biases probably counterbalanced each other to some degree. Furthermore, many of these potential biases were presumably reduced through analytic controls. For example, school enrollment at baseline was always controlled in estimating

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[4] The adjusted percentages of teens with a positive school status at 12 months was 59% for experimentals and 48% for comparisons among the 737 teens interviewed at 12 months. This is a difference of 11 percentage points (a 23% increment for the experimentals), significant at the .001 level. Using exactly the same specification for the 675 teens who were re-interviewed at 24 months, the adjusted percentages with a positive school status were 56% and 49%, for experimentals and comparison teens, respectively. This difference of only 7 percentage points (a 14% increment) was significant at the .05 level.

TABLE 2.4

SUMMARY OF POTENTIAL BIASES AFFECTING FINAL RESULTS, BY HYPOTHESIZED DIRECTION OF THE BIAS

TYPE OF BIAS	Would Make Program Appear More Effective Than It Really Is	Would Make Program Look Less Effective Than It Really Is
HISTORY		
High rate of comparison teen enrollment in a teen parent program post-baseline		X
SELECTION		
Higher rate of school enrollment of comparison teens at baseline		X
Longer exposure among comparison teens to special school programs for teen mothers (Sample I)		X
Older mean age of first birth among comparison teens		X
Fewer days at risk to a repeat pregnancy among comparison teens		X
Fewer times dropped out of school among comparison teens (Sample II)		X
Strong long-term links to service providers among comparison teens		X
Failure to interview Sample II experimental teens from less stable backgrounds	X	
ATTRITION		
Experimental teens less likely to be re-interviewed; less likely to be living at home at first interview	X	
Pregnant teens less likely to be re-interviewed		X
Experimental teens with better school outcomes exited study between 12- and 24-month interview		X

program impacts. However, unmeasured characteristics that might lead to baseline school enrollment and to better eventual outcomes (such as motivation, ability, or family supports) were not necessarily controlled by simply removing the effects of baseline school status. In reviewing Table 2.4, it seems reasonable to conclude that, to the extent that any biases remained, they were probably ones that would dilute or mask positive program impacts. Given the characteristics of the comparison teens--particularly their high rates of school attendance at baseline--the likelihood of overestimating positive program impacts seems remote.

G. Description of the Research Sample

Before turning to the results obtained in this impact analysis study, some basic descriptive information about the research sample is presented in this concluding section. This descriptive portrait focuses on two aspects of the sample: demographic changes over time and demographic differences among ethnic groups. Ethnic differences are highlighted here and in the presentation of descriptive information in subsequent chapters because (a) ethnic differences were more substantial than differences based on other characteristics such as age or initial parity and (b) comparisons with other samples will be enhanced, since most studies are either restricted to a single ethnicity or similarly present information according to ethnic group. Some information on sample characteristics within various other subgroups is presented in Tables E.1 to E.5 of Appendix E.

At baseline, the teens in the impact analysis study were young, economically disadvantaged, and mostly unmarried (for more detail, see Polit *et al.*, 1982). Table 2.5 presents some demographic information

TABLE 2.5

SELECTED DEMOGRAPHIC CHARACTERISTICS OF THE IMPACT ANALYSIS SAMPLE AT BASELINE, 12-MONTH AND 24-MONTH INTERVIEWS[a]

Demographic Characteristic	Baseline Interview		12-Month Interview		24-Month Interview	
	Sample I Teens	All Teens	Sample I Teens	All Teens	Sample I Teens	All Teens
Mean Age		16.3		17.5		18.5
Percent Married		6.4		10.9		11.4
Percent Separated or Divorced		2.3		3.5		6.1
Percent Living in Household With Mother Present	70.0	—	59.5	57.5	44.9	47.1
Percent Living With Both Parents	17.2	—	18.2	17.0	11.8	13.3
Percent Living With Husband or Boyfriend	18.6	—	25.5	22.7	26.4	22.7
Mean Number in Household	5.4	--	5.4	5.5	5.0	5.1
Percent Living in an AFDC Household		65.6		71.7		70.9
Percent Living in Household With Income <$500 per Month	56.6	--	43.2	41.7	41.9	43.4
Percent Still in Contact With Father of Their (Youngest) Child	70.6	--	66.4	67.4	58.3	58.6

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]When baseline information could be obtained retrospectively for Sample II respondents, the information shown is for the entire impact analysis sample (N=675). When baseline information was available only for Sample I respondents, baseline figures are shown only for Sample I (N=383) and follow-up information is shown both for Sample I and the aggregated impact analysis sample separately.

about the subjects at baseline, 12-months post-baseline, and 24-months post-baseline. Over the two-year study period, the percentage of teens who were married nearly doubled, and the percentage of teens who were divorced or separated nearly tripled, but overall most teens had never been married.[5] By the time of the 24-month interview when these teens were, on average, 18 and a half years old, the majority no longer lived with their parents. Despite the relatively low rate of marriage among these teens, about one in four lived with a male partner. More than half of the teens said they were still in contact with the father of their youngest child at the 24-month follow-up. The percent of teens living in an AFDC household remained fairly stable over the 24-month period.

Despite the fact that the research sample was fairly homogeneous with respect to age, economic background, and parenting status, there were sizable ethnic group differences at the time of the final interview. As shown in Table 2.6, nearly one-fourth of the Mexican American and white teens were married, but fewer than 10% of the Puerto Rican and black teens were married. Black teens were most likely to be living with their mothers, while white teens were most likely to be living away from their mothers, quite often with a male partner. On the other hand, white teens were least likely to be in touch with the fathers of their youngest child or to be getting financial assistance from them.

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[5] Because so few respondents were married, it was not possible to examine program impacts separately for married and unmarried teens, despite evidence suggesting different outcomes for teen mothers differing in marital status.

TABLE 2.6

SELECTED DEMOGRAPHIC CHARACTERISTICS AT 24-MONTH INTERVIEW, BY ETHNICITY

Demographic Characteristic	Black Teens	Mexican American Teens	Puerto Rican Teens	White Teens	All Teens[a]
Mean Age	18.5	18.5	18.5	18.6	18.5
Percent Married	3.5	23.9	9.2	22.0	11.4***
Percent Separated or Divorced	1.9	9.2	12.6	8.5	6.1***
Percent Living With Mother	57.8	37.8	42.0	27.1	47.1***
Percent Living With Both Parents	12.5	14.6	10.9	22.0	13.3
Percent Living With Husband or Boyfriend	9.6	39.0	21.0	49.2	22.8***
Mean Number of Household Members	5.4	5.2	4.9	4.5	5.1*
Mean Age in Months of Index Child[b]	25.9	24.6	23.5	26.2	25.2*
Percent Living in an AFDC Household	76.8	51.2	88.2	57.6	70.9***
Percent Living in Household With Income >$800 per Month	17.5	26.6	35.1	53.6	26.6***
Percent Still in Contact With Father of Their (Youngest) Child	61.1	60.2	55.9	47.3	58.6
Percent Receiving Financial Assistance From Father of (Youngest) Child	35.2	43.7	40.0	24.5	36.8
Number of Respondents	313	164	119	59	675

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at 24 months after baseline.

NOTES: [a]The last column presents information for all teens in the sample, including 20 teens whose ethnicity was not black, Mexican American, Puerto Rican, or white.

[b]The index pregnancy was either the pregnancy in progress at the time of the baseline interview or the most recently terminated pregnancy at baseline.

*The group difference is statistically significant at the .05 level.

**The group difference is statistically significant at the .01 level.

***The group difference is statistically significant at the .001 level.

The sample as a whole continued to live in conditions of poverty at the 24-month interview. The majority of respondents in each ethnic group lived in a household in which at least one member received AFDC. Most teens lived in households where the monthly income was under $800, supporting an average of 5.1 persons. Poverty was most severe for black teens, only 18% of whom had household incomes exceeding $800 monthly.

CHAPTER 3

PROGRAM PARTICIPATION AND SERVICE RECEIPT

Despite the fact that hundreds of agencies offer services to teenage mothers and pregnant teens, there is relatively little published information about the teens' actual experiences in special programs. This absence of information about the functioning of a teen parent program was recently referred to in a report on services to this population. McGee (1982: 59) quoted a project director who said, "We want to know what works, at what cost, and how it was done. We want specific details. We are hungry for concrete information." The implementation reports for Project Redirection (Branch *et al.*, 1981, 1984) provide abundant documentation concerning the operation of the demonstration. This chapter supplements those reports by describing the Project Redirection experience from the teens' perspective. This chapter also examines the very important issue of service utilization by experimental and comparison teens. This issue is important for two reasons. First, the degree to which the experimental teens made use of Redirection's various services is critical to understanding program successes or failures. If program participants were not receiving services, then impacts cannot reasonably be expected. Second, the extent to which *comparison* teens were served affects our interpretation of what this impact analysis accomplished: an evaluation of an intervention versus no intervention, or a comparison of treatment modalities.

A. Length of Enrollment in Project Redirection

At the time of the 24-month follow-up interview, all but three teens in the experimental group had terminated their participation in Project Redirection. The mean length of enrollment for the experimental group as a whole was 11.6 months.[1] Length of enrollment varied according to the background characteristics of the teens, as shown in Table 3.1. Black teens, on average, were enrolled for over a year, while white teens were enrolled an average of under ten months. In terms of the sites, program enrollment tended to be longest for Harlem teens and shortest for Riverside teens, but the site differences were not statistically significant. The 3-month difference between Sample I and II teens' length of enrollment, however, was statistically significant. In light of the fact that the program had more specific exit criteria during Phase II of

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[1] The mean length of enrollment reported in the final implementation report (Branch _et al._, 1984) was 8.5 months. There are several plausible explanations for the differences reported here and in the implementation report. First, the samples are somewhat different (e.g. MDRC's report includes the 80 Sample II teens never interviewed in this study). Second, the data here are based on self-reports rather than management records. Thus, differences could reflect memory lapses, different definitions of what constituted termination, or administrative errors. Finally, in the implementation report, length of enrollment was calculated for teens no longer enrolled by the end of 1982, and excludes those still in the program. Consequently, the estimate may be weighted toward participants with a relatively shorter stay in the program.

TABLE 3.1

LENGTH OF STAY IN PROJECT REDIRECTION FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Mean Number of Months Enrolled	Standard Deviation
Black Teens	(N=149)	12.8**	9.0
Hispanic Teens	(N=125)	10.6	6.5
White Teens	(N= 27)	9.9	6.7
Teens Age 15 or Younger at Baseline	(N=111)	12.1	10.1
Teens Age 16 or Older at Baseline	(N=194)	11.4	6.3
Teens Pregnant at Baseline	(N=175)	11.1	6.2
Teens not Pregnant at Baseline	(N=130)	12.4	9.7
Teens in School at Baseline	(N=139)	11.8	6.3
Teens not in School at Baseline	(N=166)	11.5	9.1
Teens With Work Experience at Baseline	(N=187)	12.1	6.2
Teens With no Work Experience at Baseline	(N=118)	10.9	9.9
Boston Teens	(N= 45)	11.0	6.0
Harlem Teens	(N= 82)	12.9	7.0
Phoenix Teens	(N=140)	11.3	9.0
Riverside Teens	(N= 38)	10.8	7.4
Sample I Teens	(N=175)	12.9**	8.8
Sample II Teens	(N=130)	9.9**	6.4
All Teens	(N=305)	11.6	7.8

SOURCE: Tabulations are from AIR interviews with experimental group members in Samples I and II at 24 months after baseline.

NOTES: Tests of statistical significance were performed for the specified subgroup and its relevant contrast (e.g. Black teens with white and Hispanic teens).

**The group difference is statistically significant at the .01 level.

operations (including termination after 18 months of enrollment or at the 19th birthday, whichever occurred first) this finding was not too surprising. However, further analysis revealed that the difference between the two samples is probably not attributable exclusively to formal exit criteria. Some 16% of the Sample II teens, compared with 8% of the Sample I teens, were enrolled in Project Redirection for fewer than six months.

When length of program enrollment was regressed on background characteristics, several variables emerged as important predictors of length of stay (the full regression tables for this and subsequent regression analyses are presented in Appendix E). The single most powerful predictor was the time that the teen enrolled, as shown in Table E.35. The later she enrolled, the shorter her length of participation. This enrollment variable was more powerful than the Sample I/Sample II dichotomy in explaining different lengths of enrollment. This effect could reflect changes in the type of teen recruited to the program at later stages, changes in program staff, operations or funding, changes in the world at large, or some combination of these three influences. With the time of enrollment controlled, several other variables continued to influence program stay. Teens enrolled for longer periods tended to be black, to have had some pre-baseline work experience, to come from smaller families, to have been older when they first gave birth, and to be younger at baseline. The regression model explained 37% of the variance in length of program enrollment.

Overall, length of participation in the Redirection program compared favorably with that for clients in projects funded by the federal Office of Adolescent Pregnancy Programs (OAPP). OAPP is the largest single source of funding for services to adolescent parents; a total of almost 8,000 pregnant and parenting teens were served in 38 individual projects funded by OAPP in 1980-1981. Among teens who entered the OAPP projects while pregnant, 43% were active for fewer than six months and an additional 25% participated between six and twelve months (Burt et al., 1984). Among the pregnant teens entering Project Redirection, only 27% terminated within six months of enrollment and only 54% were enrolled for less than one year. Among the teens who entered the OAPP-funded projects after becoming a mother, the majority (57%) became inactive in the first six months. Among participants who entered Project Redirection as mothers, only 25% terminated within the first six months.

In summary, the demonstration can be said to have successfully recruited and retained a targeted group of teens for a reasonably long period of enrollment. Program retention appears to have been easier in the earlier months of program operations than in the later months. Black teens were especially likely to stay enrolled for a year or more, but because site is confounded with ethnicity (the majority of black teens were from Harlem), either site or ethnic differences could account for variations in program retention.

B. Teens' Views About Project Redirection

The overall reaction of program participants to their experiences in Redirection was positive. Among the experimental teens interviewed at 24 months post-baseline, 52% said they were very satisfied with Project Redirection, and an additional 37% reported being fairly satisfied. About half the teens said that the program had been helpful to them in many ways. Only 8% said Redirection had not been very helpful. Teens in Samples I and II did not differ in their self-reported satisfaction with the program.

The majority of teens in both samples described their involvement in the program as either very active (31%) or fairly active (43%). When asked their reasons for not participating more actively, the most commonly cited reasons were transportation problems, child care issues, scheduling difficulties, illness, and lack of interest. The following excerpts from the interviews are typical comments regarding inactive participation:

> "With the bus system I couldn't get there on time after getting out of school" (Phoenix teen).
>
> "I was involved with my daughter and taking care of her" (Harlem teen).
>
> "I had to babysit for my kid plus I had to cook for my family" (Boston teen).
>
> "I was working and didn't have too much time" (Riverside teen).
>
> "When I started I was having problems with the pregnancy and I couldn't go because of pains" (Phoenix teen).
>
> "I was unhappy quite a bit with the predicament with my family. I just didn't feel like going" (Phoenix teen).
>
> "I was just too tired" (Riverside teen).

The teens often cited similar reasons for ending their participation in the program, although some additional reasons were also common. Termination by the program was a frequently mentioned reason for not staying, particularly in Harlem and Phoenix. Harlem teens tended to mention termination due to the age limit, while Phoenix teens more often said they were terminated because they could only stay so many months. Of the 95 Phoenix teens who explained why they left the program, 34% said the program terminated them. Of the 75 Harlem teens who responded, 51% said that they were terminated by the program.[2]

Teens in the experimental group were also asked about the component of the program that they felt had been most helpful to them. The responses at 12 and 24 months post-baseline are shown in Table 3.2. Parenting education was perceived to be the most helpful program component at both follow-up interviews. Other components viewed by a sizable percentage of teens as being most helpful were the community woman (especially for Sample I teens), employment workshops, education (especially for Sample II teens), and personal counseling.

Direct questioning about the community woman component at the final interview generally elicited favorable responses. Nearly 75% of the teens said that while enrolled in Project Redirection they had seen their community women at least once a week; 40% said they saw her at least two or three times a week. When asked how important their community woman was to them, 47% of the teens said "very important." In discussing their community women, the teens generally mentioned how nice the women were, how comfortable they were to talk to, how helpful they were with advice and concrete assistance, and how they took the teens to various places. Some typical comments about the role that the community women played in the teens' lives include the following:

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[2] According to the final implementation report (Branch et al., 1984), only 11% of the teens left due to the mandatory exit criteria while 39% were terminated because of failure to meet program requirements. Possible reasons for the discrepancies in the implementation and this report include all of those cited above in Footnote 1. The truncation problem in the implementation report calculations would tend to underestimate the percentage of teens leaving because of turning 18 or because 18 months of enrollment had elapsed. Also, not all teens in this survey specified the reason for program termination.

TABLE 3.2

PARTICIPANTS' PERCEPTIONS REGARDING MOST HELPFUL PROJECT REDIRECTION COMPONENT AT 12-MONTHS AND 24-MONTHS POST-BASELINE

Program Component	Percentage Distribution of Teens	
	Perception at 12-Months Post-Baseline	Perception at 24-Months Post-Baseline
Parenting Education	29.0	31.8
Community Woman	21.6	15.6
Employment Workshops	12.4	10.4
Education	10.0	13.6
Personal Counseling	8.9	15.6
Family Plannning	5.8	3.2
Nutrition	3.9	1.3
Health	3.5	4.5
Recreation	3.5	4.5
Child Care	1.5	1.2
Total	100.0	100.0
Total Number of Respondents	259	154

SOURCE: Tabulations are from AIR interviews with experimental group members in Samples I and II at 12 and 24 months after baseline.

NOTES: The totals may not add to 100.0 percent due to rounding error.

> "We were able to talk about things and she took me places. She was just a real good friend" (Phoenix teen).
>
> "I liked my community woman. She was very compassionate and concerned. I really liked her a lot" (Harlem teen).
>
> "She was somebody I could unload everything on. We did a lot together--shopping, movies, dinner. She's there to help you all the time" (Riverside teen).
>
> "I like her a lot, and she understands me. I always share my problems with her and she always finds a solution" (Boston teen).

Not all of the teens had had such positive experiences with their community women. Some 18% said their community women had not been very important to them. These teens tended to complain about accessibility to their community women, problems relating to turnover, and lack of compatibility. The following comments are typical of the teens' complaints:

> "There wasn't much for me to talk about with her. I don't even remember her name" (Boston teen).
>
> "I really handled all my problems myself. There were a lot of things I wouldn't talk to her about" (Phoenix teen).
>
> "She wasn't important because she didn't really spend a lot of time with me" (Harlem teen).
>
> "They messed up the community woman thing. I didn't have one for a long time. I was married and my life was put together. My first community woman quit and they were short on community women" (Riverside teen).

The teens' families tended not to have much involvement in the program. Some 32% of the teens reported that a parent had been to the program offices or participated in a Redirection activity. However, three-fourths of the teens in both samples felt their parents were involved the right amount. When asked how their families felt about their participation in Project Redirection, most teens reported that their families encouraged it and thought it was helpful. The following are typical comments the teens made about the attitudes of family members:

> "They liked it. They felt I was finally doing something with myself" (Riverside teen).
>
> "They think it's a good thing because there are people there who know a lot about how to care for a baby. They think that giving help to young mothers who don't have much experience is a good thing" (Boston teen).
>
> "They felt real good about it. They thought it would help me" (Riverside teen).
>
> "My mother was very interested and happy about it. She was the one who got me in it" (Harlem teen).

The majority of experimental group teens (64%) felt that the program had influenced their decision to stay in or return to school. As suggested by the following comments, the program's influence ranged from support and encouragement to more concrete forms of assistance:

> "My community woman encouraged me to stay in school and make future plans" (Riverside teen).
>
> "They told me to keep trying, keep going, and encouraged me to get something out of my life" (Boston teen).
>
> "They told me how I could get day care if I wanted to stay in school" (Phoenix teen).
>
> "They got me into a secretarial training program" (Harlem teen).

Nearly half of the teens in both samples said that the program influenced them to get or look for a job. The teens mentioned the program's encouragement and advice, career counseling, employment training, and job search assistance as elements of this influence:

> "They gave me the training I needed" (Phoenix teen).

> "They referred me to different leads for employment. They also prepared us for getting a job, gave classes on how to fill out an application" (Boston teen).
>
> "They helped me look for a job. They helped me find out what I did best" (Riverside teen).
>
> "They told me the sooner I got a job and became independent of welfare I would feel better about myself" (Harlem teen).

In summary, the majority of teens reported satisfaction with the program and felt the program had had a positive influence on them. The community woman component appears to have been successfully implemented, with teens generally having weekly interactions with their community woman and usually reporting a caring and satisfying relationship.

C. Service Utilization by Experimental and Comparison Teens

This section examines the use of various services by teens in the experimental and comparison groups. Two main questions are addressed: (1) Did experimental group teens receive the services that Project Redirection was designed to deliver? and (2) Did experimental teens receive substantially more services than other teen parents from similar backgrounds (the comparison group)? We first examine service utilization in the experimental group as reported in the 12- and 24-month interviews.

Table 3.3 presents information on Redirection participants' use of, and need for, 12 selected services as reported at the 12-month interview (left panel) and 24-month interview (right panel). Between baseline and the 12-month interview (when all experimental teens were enrolled in the program), the majority of experimental teens had received parenting education, medical care for themselves and their babies, birth control counseling, nutrition education and some form of job/employability training. Column 2 shows that for all services except those that are

TABLE 3.3

UTILIZATION OF SELECTED SERVICES BY EXPERIMENTAL TEENS IN THE POST-BASELINE PERIOD

Type of Service	Baseline to 12-Month Interview[a]			12-Month to 24-Month Interview[b]		
	Percent Receiving Service	Percent Receiving From Redirection	Percent Needing Service Who Did Not Receive It	Percent Receiving Service	Percent Receiving From Redirection	Percent Needin Service Who Di Not Receive It
Parenting Class	64.3	49.1	3.7	38.0	27.9	7.0
Tutoring for School Work	9.4	5.6	26.3	10.0	6.9	20.8
Medical Care for Baby	96.1	0.7	1.8	95.4	0.0	1.5
Medical Care for Self	82.2	0.4	4.6	80.6	0.0	5.4
Recreational Program	37.3	29.1	19.4	26.2	12.4	20.2
Birth Control Counseling	73.7	35.4	5.1	44.6	11.5	3.8
Educational Counseling	41.3	33.3	17.1	35.4	26.9	20.0
Nutrition Education	56.1	33.3	6.5	41.5	19.2	6.9
Personal Counseling	43.7	29.1	9.8	30.8	18.5	16.2
Housing Assistance	15.0	8.8	26.1	14.6	5.4	33.8
Pregnancy Counseling	31.7	15.8	4.5	19.2	6.2	3.1
Job Employability/Training[c]	65.2	43.6	NA[c]	38.0	12.3	30.0

SOURCE: Tabulations are from AIR interviews with experimental group members in Samples I and II at 12 and 24 months after baseline.

NOTES: The percentages do not add to 100.0 percent because respondents could use multiple services.

[a] The percentages are based on responses from 285 teens interviewed 12 months post-baseline. Respondents were asked if they had used the specified services in the preceding 12 months.

[b] The percentages are based on responses from 130 teens who were still in the Redirection program *after* the 12-month interview.

[c] In the 12-month interview, respondents were asked if they had received job training in seven specific areas (e.g., how to complete a job application, how to find a job). Respondents were coded as having received job training if they received any of the seven types of training. The 12-month interview did not ask about the need for job training. In the 24-month interview, job training was handled like any of the other services appearing on this list.

health-related, Redirection was a major direct provider of these services. For example, 41% of these teens obtained educational counseling in the 12 months following enrollment, and of these 81% (33% ofthe entire sample) had obtained the service from Redirection. Finally, the left panel of Table 3.3 also shows that, while in the program, the experimental teens' perceived need for services they were not obtaining was generally low. The greatest perceived unmet needs reported at the 12-month interview were tutoring and housing assistance.

At the time of the 24-month interview, 130 experimental teens (43%) had been in the Redirection program in the preceding year and could report on services provided or brokered by the program. The right panel of Table 3.3 shows the percentage of these 130 teens having obtained the 12 specified services from any source. Service receipt had declined between the 12- and 24-month interviews for all services except medical care and housing assistance. Despite these declines, substantial percentages of these experimental teens reported having obtained many of the specified services.

Except for services related to health, the Redirection program was still a major service provider to those teens still enrolled after 12 months. For example, 73% of the teens who obtained parenting education (28% of all respondents) received it from the program directly. Although not shown in Table 3.3, the 24-month interviews also obtained information on receipt of services on referral from Project Redirection. It was learned that the program was responsible (either directly or indirectly) for serving at least half of those who obtained services in the areas of parenting education, tutoring for school work, educational counseling, personal counseling, housing assistance, and job/employability training.

According to the teens' reports at 24 months post-baseline, the areas for which unmet needs were highest were housing assistance and job training; about one-third of the sample felt such services were needed but not received.[3] The data in Table 3.3 indicate that, while the experimental teens were enrolled in Project Redirection, the program was indeed delivering (directly or indirectly) a comprehensive range of services.

The second service-related issue to be examined was the difference in service receipt between experimental and comparison teens. Table 3.4 shows, in the left panel, the use of services from baseline to the 12-month interview for both experimental and comparison teens. During this period all experimental teens were enrolled at some point in Project Redirection. The data indicate that a significantly higher percentage of experimental teens than comparison teens obtained 10 of the 12 specified services. The most noteworthy group differences were in the areas of educational counseling, job training, personal counseling, parenting education, and recreational activities. Despite these significant differences, however, substantial percentages of comparison teens received many of these services, including service in an area that presumably made the Redirection program distinctive, job training. Also noteworthy are the high levels of medical care and birth control counseling among comparison teens.

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[3] The 175 experimental teens who were not in the program beyond 12 months reported a similar pattern of unmet service needs, although for these teens job training ranked first, with 37% saying they needed this service but were not obtaining it.

TABLE 3.4

PERCENTAGE OF EXPERIMENTAL AND COMPARISON GROUP MEMBERS RECEIVING SELECTED SERVICES SINCE BASELINE INTERVIEW

Type of Service	Percentage Using Given Service, by Group and Time Period				
	Baseline to 12-Month Interview		12 Month to 24-Month Interview		
	Experimental	Comparison	Experimental Enrolled > 12 Mos.	Experimental Enrolled < 12 Mos.	Comparison Teens
Parenting Classes	64.3***	39.6	38.0***	10.8**	18.1
Tutoring for School Work	9.4*	4.9	10.0*	5.2	5.1
Medical Care for Baby	96.1*	91.7	95.4	94.6	91.8
Medical Care for Self	82.2	78.1	80.6*	78.6	71.4
Recreational Program	37.3***	8.2	26.2***	9.2	7.9
Birth Control Counseling	73.7**	62.8	44.6	43.1	51.4
Educational Counseling	41.3***	19.4	35.4***	18.9	13.2
Nutrition Education	56.1**	45.5	41.5*	27.6	32.2
Personal Counseling	43.7***	15.9	30.8***	14.9	10.8
Housing Assistance	15.0**	7.2	14.6	13.2	9.7
Pregnancy Counseling	31.7	25.1	19.2	13.3	19.7
Job/Employability Training	65.2***	45.1	38.0***	21.3	15.9
Number of Respondents	305	370	130	175	370

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at 12 and 24 months after baseline.

NOTES: The percentages do not add to 100.0 percent because respondents could use multiple services.

Tests of statistical significance (chi-square tests) were performed between experimental and comparison groups within each of the two time periods. Significant differences are indicated next to the experimental percentages.

*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

The panel on the right in Table 3.4 shows service utilization from 12 to 24 months post-baseline for experimental and comparison teens. The service receipt pattern for experimental teens is also shown separately for those still enrolled in the program after 12 months and for those terminated prior to 12 months. Overall differences between the experimental and comparison groups continued to be significant in the final 12-month period for 5 of the 12 specified services. However, these differences were accounted for completely by differences between comparison teens and longer-term experimental teens. None of the differences between short-term enrollees and comparison teens was significant, whereas longer-term experimental teens had a significant advantage for 8 of the 12 services.[4] These data suggest that departure from the program was associated with a sharp decline in service receipt. Teens no longer in the program had a pattern of service utilization fairly similar to that of comparison teens.

From the point of view of interpreting the results of this study, a critical finding revealed in Table 3.4 is that the comparison teens were by no means an unserved or minimally served group. The majority of these teens reported at both follow-up interviews that they had obtained medical care for themselves and their infants. The majority had also received birth control counseling; in fact, between the 12- and 24-month interviews, more of the comparison than experimental teens obtained contraceptive counseling. Almost half of the comparison group had had parenting education, nutrition education, and some type of job or employability training at some point during the study period.

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[4] The difference between experimental teens enrolled more than 12 months and comparison teens was statistically significant for the following services: parenting classes, recreational programs, educational counseling, personal counseling, and job training ($p < .001$); and tutoring, medical care for self, and nutrition education ($p < .05$).

Data from the previously mentioned OAPP-funded projects is useful in putting comparison group service receipt into perspective. Table 3.5 compares service receipt among Project Redirection experimental and comparison group teens as reported at the 12-month interview with that for clients at 28 of the OAPP-funded teen parent programs with respect to services for which a comparison could meaningfully be made. This table suggests that a higher percentage of comparison teens than OAPP clients obtained many of these services, and experimental teens obtained the services at even higher rates. Although there are sufficient differences in how service receipt was measured in the two studies to warrant caution in making direct comparisons, the data in Table 3.5 nevertheless corroborate the inference that comparison group teens were a well-served group.[5]

Because of the importance of this issue, further analyses were conducted to explore the service receipt history of the comparison group teens. These analyses revealed that more than half (54%) of the comparison teens had been served by a special teen parent program in their

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[5] The percentages reported in the Burt et al. (1984) report are known to underrepresent the total amount of services that clients receive while enrolled in OAPP programs. The figures represent one year of the projects' provision of services, not a year of services per client. Thus clients enrolled only two months when the one-year "snapshot" was taken might eventually receive services not yet obtained. Another difference between how services in the two studies were measured is that the present study uses self-reports and the OAPP study is based on management information data.

TABLE 3.5

SERVICE UTILIZATION OF EXPERIMENTAL AND COMPARISON TEENS IN PROJECT REDIRECTION BETWEEN BASELINE AND 12-MONTH INTERVIEW COMPARED WITH CLIENTS OF OAPP-FUNDED PROJECTS

Service	Percentage Using Given Service, by Group		
	Redirection Experimental Group[a]	Redirection Comparison Group[a]	OAPP Project Clients[b]
Parenting Classes	64.3	39.6	27.2[e]
Birth Control Counseling	73.7	62.8	56.3[f]
Educational Counseling[c]	41.3	19.4	38.9[f]
Nutrition Education	56.1	45.5	28.2[g]
Personal Counseling	43.7	15.9	39.6[h]
Housing Assistance	15.0	7.2	3.2[h]
Job/Employability Training[d]	65.2	45.1	4.6[h]

SOURCES: [a] Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II 12 months after baseline.

[b] Tabulations are from management information system data from 28 projects funded by the Office of Adolescent Pregnancy Programs in FY 1982 (Table III-6 in Burt et al., 1984). These percentages represent one year of projects' service delivery effort, not one year of service delivery to every client.

NOTES: [c] In Project Redirection, this service included educational counseling only. In OAPP projects, this service included both educational and vocational counseling.

[d] In Project Redirection, job training services referred to career counseling, employability training, or job training. In OAPP projects, this service referred to a formal job training program.

[e] Based on pregnant teens and mothers, total N=7,417.

[f] Based on all female clients, 1,017 of whom were neither pregnant nor a parent; total N=8,434.

[g] Based on pregnant teens only, N=4,501; for all female clients the percentage was 22% of 8,434 clients.

[h] Based on all clients, including 1,074 males; total N=9,508.

own community, 80% of these during the post-baseline period. Some of these special programs were school-based, while others were under the auspices of hospitals or community-based organizations. Among those participating in a special program, the mean length of enrollment was 7.1 months. Among those enrolled in special school-based programs, a third were enrolled for two or more school semesters.

Additional information regarding the service receipt history of the comparison group was available for Sample I teens. For Sample I, information on service receipt prior to entering the study was obtained at baseline. Table 3.6 shows that, of the 12 services listed in Table 3.4, comparison teens had received significantly more services than experimental teens after background characteristics were adjusted. By the time of the 12-month follow-up, experimental teens were significantly better served than comparison teens, in both Sample I and II. The group differences were maintained, but of smaller magnitude, by the time of the 24-month interview.

Thus far the focus has been on the range of services received. Information was also gathered regarding the intensity of service receipt. Table 3.7 presents information regarding the mean number of times teens reported having obtained specified services between baseline and the 12-month interview. The picture that emerges from this table is consistent with the information reported earlier. Three important points deserve comment. First, while experimental and comparison group differences were almost all significant, comparison group teens were not typically getting "one-shot" service. This is even more apparent when teens never receiving a specified service are excluded from the calculations. For example, among the 72 comparison teens who obtained

TABLE 3.6

ADJUSTED MEAN NUMBER OF SERVICES USED BY EXPERIMENTAL AND COMPARISON GROUP MEMBERS IN SAMPLE I AND II, AS REPORTED AT BASELINE, 12-MONTH AND 24-MONTH INTERVIEWS

Sample and Time Period	Mean Number of Services Used, By Group		
	Experimental	Comparison	Difference
• Sample I Teens			
Mean Number of Services Used Prior to Baseline	3.03	3.68	- .65**
Mean Number of Services Used Between Baseline and 12-Month Interviews	5.67	4.26	1.41***
Mean Number of Services Used Between 12- and 24-Month Interviews	3.90	3.42	.48*
• Sample II Teens			
Mean Number of Services Used Between Baseline and 12-Month Interview	6.12	4.36	1.76***
Mean Number of Services Used Between 12- and 24-Month Interviews	4.16	3.49	.67**

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The number of services used is based on the respondents' self-reported use of 12 specific services in the 12 months prior to the interview.

The means have been adjusted for respondents' ethnicity, age at first birth, school status at baseline, baseline participation in a teen parent program, and highest grade completed at baseline.

These means are based on responses from 141 experimental and 200 comparison group members in Sample I and 130 experimental and 159 comparison group members in Sample II.

*The group difference is statistically significant at the .05 level.

**The group difference is statistically significant at the .01 level.

***The group difference is statistically significant at the .001 level.

TABLE 3.7

MEAN NUMBER OF TIMES TEENS RECEIVED SELECTED SERVICES BETWEEN BASELINE AND 12-MONTH INTERVIEW, BY GROUP AND LENGTH OF PROGRAM PARTICIPATION

Type of Service	All Experimental Teens	Experimental Teens, by Length of Enrollment			Comparison Teens
		< 6 Mos.	6-12 Mos.	>12 Mos.	
Parenting Classes	16.5***	10.1	18.4	18.9	8.3
Tutoring for School Work	3.3*	0.7	4.1	4.4	1.6
Medical Care for Baby	8.8**	7.1	9.1	9.7	6.9
Medical Care for Self	5.2	3.8	5.8	5.5	4.7
Recreational Program	8.4***	5.9	10.3	8.6	2.0
Birth Control Counseling	3.9**	2.7	3.1	5.2	2.5
Educational Counseling	5.5***	3.0	6.2	6.5	1.8
Nutrition Education	10.1***	6.8	11.8	10.9	6.1
Personal Counseling	7.7***	5.0	6.5	10.3	1.6
Housing Assistance	0.9**	0.2	0.7	1.5	0.2
Pregnancy Counseling	4.1	2.0	3.7	5.6	2.9
Number of Respondents	305	80	95	130	370

SOURCE: Tabulations are from AIR interviews with 675 experimental and comparison group members in Samples I and II 12 months after baseline.

NOTES: The computed means were based on all teens in the sample, including those who received no services.

Test of statistical significance (t-tests) were performed between comparison and experimental teens. Significant differences are indicated next to the experimented means.

*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

educational counseling between baseline and 12 months post-baseline (19%), the mean number of times this service was received was about nine times. The second point is that the Redirection program was clearly providing experimental teens with not only a comprehensive mix of services but also fairly intensive services. Finally, as length of enrollment increased, service intensity also increased.

In summary, there were two major types of findings from the analyses on service utilization. First, the experimental teens obtained many services from or through Project Redirection during the two-year study period. After these teens terminated the program, their service receipt dropped off sharply. Between baseline and the 12-month interview, however, service utilization was both extensive and intensive. This finding lends further evidence regarding the feasibility of the program model and provides a justification for examining whether the program had any impacts on participants' lives.

A second and equally important finding was that comparison teens were quite well served. Many were receiving the same services as experimental teens and the majority had also participated in a special teen parent program. This finding is of concern because it affects the interpretation of the impact results to be presented in the next three chapters. The resulting design is essentially a comparison of treatment modalities, with Project Redirection being compared to varied other types of service delivery of undetermined scope and nature. Furthermore, it is an insensitive comparison, since, in designing the study, sample size requirements were based on an assumption of a relatively unserved comparison group. In other words, the study design would have called for a larger sample size if it had been known how well-served the comparison group would be, since a larger sample would be needed to detect smaller rates of improvement at statistically significant levels. Thus, the findings reported here should be construed as a conservative test of program impacts. The analyses do not address the question of whether participation in this special teen parent program influenced the teens' educational, employment, or fertility behaviors over and above an absence of services, but rather, whether such participation resulted in improved outcomes relative to other services.

CHAPTER 4

PROGRAM IMPACTS ON FAMILY PLANNING AND HEALTH VARIABLES

Self-sufficiency is a formidable goal for disadvantaged young mothers who have dropped out of school during or after their pregnancies. Such young women face multiple obstacles in obtaining employment--lack of educational credentials, little work experience, child care problems, and perhaps the absence of working role models. Self-sufficiency becomes even more difficult for these young women if they have early subsequent pregnancies. Bane and Ellwood (1983) recently found that having a preschool child is less of a deterrent to leaving AFDC through work than the number of children the woman has. Other investigators have also concluded that the negative consequences associated with teenage parenthood are exacerbated by early subsequent births (e.g. Jekel et al., 1975; Furstenberg, 1976).

Early repeat pregnancies among teenage mothers occur at a disturbingly high rate. Reports based on large-scale surveys indicate that about one out of five teenage mothers--regardless of ethnicity--becomes pregnant again within 12 months of delivering her first child. For example, in the 1979 Survey of Young Women aged 15-19 from metropolitan areas, 20% of black mothers had a repeat pregnancy within 12 months and 39% had one within 24 months (Koenig and Zelnik, 1982). Ford (1983), using data from the National Survey of Family Growth, reported a 12-month repeat pregnancy rate of 18% for black teens under 18 at first birth. Testa (1983) similarly found a repeat pregnancy rate of 21% after 12 months and 42% after 24 months for black teens under age 18 who were AFDC recipients in Illinois.

Data from birth certificate records suggest that repeat pregnancies are probably underreported in surveys. In 1981, according to birth certificates from 49 states and the District of Columbia, 61% of black teens and 63% of white teens who ever had a repeat live birth by age 19 did so within 23 months of an earlier birth (National Center for Health Statistics, unpublished data). Further discussion of rates of repeat pregnancy in other samples is presented in Appendix D.

One of the goals of Project Redirection, and for most teen parent programs, is to help teens postpone a subsequent pregnancy. This analysis of program impacts begins by focusing on repeat pregnancy and contraceptive utilization because teens' educational and employment behaviors are likely to be shaped by their avoidance of an early subsequent birth.

This chapter, and the two that follow, are organized to first present an overview of the topic at hand and then to pursue the question of program impacts with respect to relevant outcomes. The descriptive introductory sections examine two main themes: changes over the two-year study period for the entire study sample and ethnic group differences. Differences between experimental and comparison teens are then examined in the impact analysis sections. Four content areas are included in the present chapter: fertility, contraceptive use, the health of infants born subsequent to baseline, and child care.

A. Fertility Behaviors and Attitudes: An Overview

Despite their youth at baseline (mean age of 16.4), nearly one out of five teens had already had more than one pregnancy, as shown in the first panel of Table 4.1. By the time of the 24-month interview, when these teens were 18.3 years old on average, more than half (56%) had had two or more pregnancies. Between baseline and the final interview, nearly one third of the sample had had a second or higher-order live birth. On average, these teens had given birth to 1.4 children by the time of the 24-month interview. This average can be projected to rise to 1.5 when the 13% who were pregnant when interviewed deliver.

Among the 89 teens who were pregnant at the 24 month interview, only two (2%) indicated an intention to seek an abortion. Given the high rates of pregnancy in this group, the percentage of teens who have ever had an abortion is low. At baseline, about 5% of the teens had terminated an earlier pregnancy in abortion. By the 24-month interview, the percentage had risen to 13%. For the sample as a whole, the abortion rate (number of abortions per 1,000 live births) was 112, which is approximately 10% of all pregnancies. For the U.S. as a whole, vital statistics records indicate that the percentage of pregnancies (excluding miscarriages) that were terminated by abortion for women aged 15 to 19 was 41% in 1980 (Henshaw and O'Reilly, 1983). Thus, it appears that this sample was considerably more likely to terminate their pregnancies in a live birth than is true for teenagers as a whole, perhaps in part because the cost of an abortion is prohibitive to these economically disadvantaged teens, and perhaps because of the selective nature of the sample (i.e., all girls were or were planning to become mothers). However, it is also likely that

TABLE 4.1

SELECTED FERTILITY-RELATED VARIABLES FOR THE IMPACT ANALYSIS SAMPLE AT BASELINE, 12-MONTH AND 24-MONTH INTERVIEWS[a]

Selected Variable	Baseline Interview		12-Month Interview		24-Month Interview	
	Sample I Teens	All Teens	Sample I Teens	All Teens	Sample I Teens	All Teens
Percent Pregnant on Interview Date		61.8		9.4		13.2
Mean Number of Pregnancies		1.20		1.40		1.73
Percent with More Than One Pregnancy		18.4		31.7		55.6
Mean Number of Live Births		0.48		1.10		1.35
Percent with More Than One Live Birth		4.2		11.7		32.5
Percent with One or More Abortions		4.9		9.0		13.2
Percent Wanting Another Pregnancy	73.3	----	51.4	57.7	56.0	59.3
Mean Number of Months Desired to Next Pregnancy	64.4	----	53.1	52.5	50.6	51.1
Mean Number of Children Wanted in All	2.5	----	2.4	2.3	2.6	2.5

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]When baseline information could be obtained retrospectively for Sample II respondents, the information shown is for the entire impact analysis sample (N=675). When baseline information was available only for Sample I respondents, baseline figures are shown only for Sample I (N=383) and follow-up information is shown both for Sample I and the aggregated impact analysis sample separately.

some underreporting of abortions occurred among these teens, as has been found in other studies.[1]

Table 4.1 indicates that, at baseline, most teens in Sample I (73%) wanted another pregnancy, but they wanted to space their next child by an average of about five years (64.4 months). Given that most teens had a subsequent pregnancy within two years of baseline, it seems likely that most of these repeat pregnancies were unintended. This is consistent with data from the National Survey of Family Growth, in which it was found that 82% of the repeat pregnancies to black teenagers were unplanned (Ford, 1983).

The data in Table 4.1 suggest that, unless the teens in the study sample improve their fertility control, they will have more children than they want. At all three interview periods the average number of children desired was about 2.5, which is somewhat higher than the expectations of a national sample of 18-19 year olds in 1978 who said they wanted 2.0 children (U.S. Bureau of the Census, 1979). At an average age of just over 18, teens in the research sample had already had more than half of the total number of children they said they wanted.

Information relating to ethnic group differences for fertility variables measured at 24 months post-baseline is presented in Table 4.2.

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[1] A comparison of survey responses from the National Survey of Young Women aged 15-19 with abortion data collected from abortion providers by the Alan Guttmacher Institute revealed that black teens actually had about four times as many abortions as they reported (Zelnik _et al_., 1981, Appendix B). In data collected in the National Longitudinal Surveys of Work Experience of Youth, underreporting of abortions was even greater (Mott, 1983). It seems likely that underreporting would be of a smaller magnitude in the present study, given that all of these teens were included _because of_ a pregnancy and either actual or intended parenthood. Furthermore, there was a high degree of consistency in reported pregnancies (and abortions) at baseline and at the 12-month interview (See Appendix A). However, it is probably safe to assume that some underreporting did occur.

TABLE 4.2

SELECTED FERTILITY OUTCOMES AT 24-MONTH INTERVIEW, BY ETHNICITY

Fertility Outcomes	Black Teens	Mexican American Teens	Puerto Rican Teens	White Teens	All Teens[a]
Mean Total Number of Pregnancies	1.74	1.66	1.87	1.63	1.72
Mean Total Number of Births	1.33	1.38	1.46	1.19	1.35*
Percent With One or More Abortions	18.8	4.3	10.9	13.6	13.2***
Percent With One or More Miscarriages	6.7	9.8	11.8	11.9	8.7
Percent With a Pregnancy Subsequent to Index[b]	44.7	44.5	54.6	44.1	46.2
Percent With a Live Birth Subsequent to Index	23.3	27.4	31.9	16.9	25.2
Percent With an Abortion Subsquent to Index	9.6	2.4	7.6	10.2	7.4**
Percent With a Miscarriage Subsequent to Index	3.2	6.7	3.4	10.2	4.6
Number of Respondents	313	164	119	59	675

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at 24-months after baseline.

NOTES: [a] The last column presents information for all teens in the sample, including 20 teens whose ethnicity was not black, Mexican American, Puerto Rican, or white.

[b] The index pregnancy was either the pregnancy in progress at the time of the baseline interview or the most recently terminated pregnancy at baseline.

*The group difference is statistically significant at the .05 level.

**The group difference is statistically significant at the .01 level.

***The group difference is statistically significant at the .001 level.

The four ethnic groups had a similar mean numbers of pregnancies, but Puerto Rican teens had a higher mean number of live births, while whites had a lower mean number. The lower number of births among the whites largely reflects the fact that they delayed their subsequent pregnancies somewhat longer than other ethnic groups. Among those teens who had a pregnancy subsequent to the index pregnancy (the pregnancy in progress or most recently terminated at baseline), the interval between delivery and the onset of the next pregnancy was 16.6 months for whites, compared with 14.9 months for blacks, 13.9 months for Mexican Americans and 11.9 months for Puerto Ricans. Whites were as likely as other groups, however, to have a repeat pregnancy during the two-year study period.

The only other area in which significant ethnic group differences emerged related to abortions. Only 4% of the Mexican American teens, compared with 19% of the black teens, reported having ever obtained an abortion. For teens who had a repeat pregnancy only 2% of the Mexican Americans terminated it through abortion. This difference may in part be attributable to the fact that 24% of the Mexican American teens were married at the final interview, although a similar percentage of whites (22%) were also married. Data from large national surveys suggest that Mexican Americans have less favorable attitudes toward abortion than other ethnic groups. For example, 51% of white respondents felt a woman should be able to obtain an abortion if her income were too small to support another child, compared with 41% of blacks and 34% of Mexican Americans. (Darabi _et_ _al_., 1983).

In most studies, rates of repeat pregnancy are reported relative to the date of a previous pregnancy rather than in relation to a "baseline" date. Therefore, for purposes of comparison with other research, rates of repeat pregnancy were computed for three specific times relative to the index pregnancy: 12, 18, and 24 months postpartum.

In the present study, one problem that should be kept in mind is that 12-month postpartum data were available for all teens while 18- and 24-month postpartum data were only available for teens who entered the study as mothers or who had just delivered at baseline. Thus for each of the three time intervals, the sample is somewhat different.

Table 4.3 shows unadjusted ethnic group differences in rates of subsequent pregnancies for the three postpartum intervals. Overall, 22% had a repeat pregancy within 12 months, a figure comparable to that found in the surveys cited in the introduction to this chapter. By 18 months postpartum, 36% had another pregnancy, and by 24 months, 48% of those for whom 24-month information was available had a repeat pregnancy. At all three time periods examined, Puerto Rican teens had the highest rates of subsequent pregnancies, and white teens had the lowest.

In summary, the majority of teens in the study sample had had two or more pregnancies by the end of the study. About half had had a pregnancy that began during the two-year follow-up period, despite the fact that most were pregnant at baseline. Relatively few of these teens were opting for abortions, choosing instead to add to their families. These teens have apparently not been successful in accomplishing their stated goals of postponing other pregnancies beyond their teen years.

B. Redirection Impacts on Fertility

Given the evidence regarding the adverse effects of early repeat pregnancies for teenage mothers, a major program objective was to

TABLE 4.3

REPEAT PREGNANCIES AT SELECTED INTERVALS SUBSEQUENT TO INDEX PREGNANCY, BY ETHNICITY

Subsequent Pregnancy Variable	Black Teens	Mexican American Teens	Puerto Rican Teens	White Teens	All Teens[a]
Percent With Subsequent Pregnancy 12 Months Postpartum[b]	20.4 (313)[c]	21.3 (164)	32.8 (119)	15.3 (59)	22.1* (675)
Percent With Subsequent Pregnancy 18 Months Postpartum	33.6 (277)	35.5 (146)	44.8 (96)	32.1 (53)	35.5 (591)
Percent With Subsequent Pregnancy 24 Months Postpartum	49.1 (171)	42.7 (75)	65.5 (75)	32.4 (34)	48.4* (347)
Mean Number of Months to Subsequent Pregnancy	14.9 (140)	13.9 (73)	11.9 (65)	16.6 (26)	14.2* (312)

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] The last column presents information for all teens in the sample, including 20 teens whose ethnicity was not black, Mexican American, Puerto Rican, or white.

[b] Postpartum refers to period following the index pregnancy. The index pregnancy was either the pregnancy in progress at the time of the baseline interview or the most recently terminated pregnancy at baseline.

[c] The number in parentheses indicates the number of teens about whom the information was available. Since some teens were pregnant at baseline, the pregnancy status of all teens at 18 and 24 months postpartum could not be ascertained during the 24-month post-baseline follow-up.

*The group difference is statistically significant at the .05 level.

**The group difference is statistically significant at the .01 level.

encourage participants to postpone a subsequent pregnancy. This section addresses the question of whether or not participation in Project Redirection resulted in improved outcomes in the area of fertility during the 24-month study period relative to comparison teens.

In the analyses of program impacts in the fertility area, various background and baseline variables were controlled using statistical procedures (See Appendix B). Included in the analyses were such variables as school status at baseline, pregnancy status at baseline, number of baseline pregnancies, age at first birth, ethnicity and highest grade completed at baseline. These variables were chosen as covariates because they have been found in other studies to have an effect on contraceptive use or subsequent pregnancies among teenagers. In the present study, the chosen covariates had a zero order correlation with the outcome variables or with the participation variable (or both).

In addition to the covariates mentioned above, variables were introduced to control for the fact that entry into the study occurred at an arbitrary date unrelated to the onset or termination of the index pregnancy. About 60% of the sample was pregnant at the time of the initial interview, and so had a short period of time at-risk to a subsequent pregnancy at the 12-month interview. Other teens had already been at-risk for many months upon program entry. Teens in the experimental group had a significantly longer period at-risk than comparison group teens. An additional complication arose as a result of the fact that the interval between the baseline and follow-up interviews was significantly longer for experimentals, thereby magnifying group differences with respect to length of time at-risk.

Controls for these two time-related variables were needed. Control was introduced in three ways. First, two variables were created to correspond to these two intervals. The at-risk period was defined as the number of days between baseline and the termination of the index pregnancy (a negative number for teens who entered the study pregnant and a positive number for those who entered as mothers). The inter-interview period was the number of days between baseline and the follow-up interviews. These variables were then used as covariates in regression analyses.

The second approach was to hold the intervals constant by comparing rates of repeat pregnancy in the two groups at fixed intervals after the termination of the index pregnancy. These outcomes, defined as a repeat pregnancy within 12, 18, and 24 months postpartum, were then regressed on background characteristics and program participation. In these analyses, controls were introduced to adjust for varying time intervals between the termination of the index pregnancy and entry into the program. The difficulty with this approach, as indicated in the previous section, was that for the teens who entered the study pregnant, 24-month postpartum data were not available at the 24-month interview. Another problem was that for some teens, 12-months postpartum occurred prior to baseline (i.e. even before experimental teens entered the program).

The third approach was to use life table analyses (survival analyses) to examine group differences longitudinally. Since the results of the three approaches led to similar conclusion with regard to program impacts, only the first two are discussed in this chapter. The life table analysis is discussed in Appendix B.

The results of the analyses in which the first approach was used are summarized in Table 4.4. This table presents group differences for selected fertility variables, after controlling relevant background characteristics. The full regression tables for these outcomes are presented in Appendix E.[2]

The first outcome is the percentage of teens who had had a repeat pregnancy 12 months after baseline. A significantly higher percentage of comparison (22%) than experimental (14%) teens had had a repeat pregnancy during the first year of the study. By 24 months after baseline, however, the group difference was no longer significant: nearly half the teens in both groups had a subsequent pregnancy.

Table 4.5 shows that program impacts on subsequent fertility at the 12-month interview were not restricted to a particular subgroup, but was a relatively pervasive phenomenon. The most powerful impacts were observed for black teens and teens who entered the program as mothers. In both cases, the repeat pregnancy rate was nearly twice as high for comparison teens as for experimental teens. The program was least successful for teens who were pregnant at baseline. White teens have been excluded from these subgroup analyses because their number was too small for reliable estimates.

By 24 months after baseline, teens in the experimental group still had fewer repeat pregnancies than comparison group teens in most subgroups, as shown in Table 4.6. However, the differences were generally not

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[2] The regression tables are not presented when the overall model was nonsignificant. For outcomes in this chapter, it proved not to be possible to reliably model use of birth control at last intercourse, and the riskiness of method used at last intercourse.

TABLE 4.4

SUMMARY OF ADJUSTED FERTILITY OUTCOMES, BY EXPERIMENTAL AND COMPARISON GROUP

Fertility Outcome	Adjusted Mean or Percentage, by Group		
	Experimental	Comparison	Difference
Percent With a Subsequent Pregnancy 12 Months Post-Baseline	14	22	-8*
Percent With a Subsequent Pregnancy 24 Months Post-Baseline	45	49	-4
Percent With a Subsequent Live Birth 24 Months Post-Baseline	22	29	-7+
Mean Number of Pregnancies 24 Months Post-Baseline	1.7	1.8	-.1
Mean Number of Months Between Index and Subsequent Pregnancy or Date of Final Interview	20.6	18.8	1.8*
Mean Number of Months Between Index and Subsequent Pregnancy, Teens With a Repeat Pregnancy (N=309)	14.7	13.6	1.1+
Number of Respondents	305	370	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The means and percentages in this table have all been adjusted for ethnicity, school status at baseline, pregnancy status at baseline, and number of baseline pregnancies. Various other characteristics were also controlled, but different covariates were required for different outcomes. The full regression tables are presented in Tables E.6 to E.9 of Appendix E for all 24-month outcomes. All covariates significantly related to the outcomes, as shown in these appendix tables, were controlled in deriving the figures presented in this table.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.

TABLE 4.5

ADJUSTED PERCENTAGES OF TEENS WITH A PREGNANCY SUBSEQUENT TO THE INDEX PREGNANCY AT 12-MONTH INTERVIEW, FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Adjusted Percentages, by Group		
		Experimental	Comparison	Difference
Black Teens	(N=312)	13	24	-11**
Mexican American Teens	(N=164)	7	17	-10+
Puerto Rican Teens	(N=119)	16	26	-10
Teens Age 15 or Younger at Baseline	(N=224)	10	16	- 6
Teens Age 16-17 at Baseline	(N-448)	16	25	- 9*
Teens Pregnant at Baseline	(N=417)	14	11	3
Teens not Pregnant at Baseline	(N=256)	20	38	-18**
Teens in an AFDC Household at Baseline	(N=441)	13	23	-10**
Teens not in an AFDC Household at Baseline	(N=220)	15	22	- 7
Teens in School at Baseline	(N=373)	7	12	- 5
Teens not in School at Baseline	(N=298)	23	34	- 9*
Teens With Work Experience at Baseline	(N=418)	14	23	- 9*
Teens Without Work Experience at Baseline	(N=254)	14	21	- 7
Boston/Hartford Teens	(N=114)	14	19	- 5
Harlem/Bedford Stuyvesant Teens	(N=185)	12	24	-12*
Phoenix/San Antonio Teens	(N=289)	13	21	- 8
Riverside/Fresno Teens	(N= 85)	10	26	-16+
Sample I Teens	(N=384)	15	24	- 9*
Sample II Teens	(N=288)	11	19	- 8+
All Teens	(N=672)	14	22	- 8*

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The percentages shown have been adjusted for number of months at risk to a subsequent pregnancy, ethnicity, pregnancy status at baseline, age at baseline, school status at baseline, number of times dropped out of school at baseline, and the time elapsed between baseline and the 24-month interview.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.

TABLE 4.6

ADJUSTED PERCENTAGES OF TEENS WITH A PREGNANCY SUBSEQUENT TO THE INDEX PREGNANCY AT 24-MONTH INTERVIEW, FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Adjusted Percentages, by Group		
		Experimental	Comparison	Difference
Black Teens	(N=310)	43	49	- 6
Mexican American Teens	(N=163)	44	46	- 2
Puerto Rican Teens	(N=119)	42	63	-21+
Teens Age 15 or Younger at Baseline	(N=223)	49	47	2
Teens Age 16-17 at Baseline	(N-448)	42	50	- 8
Teens Pregnant at Baseline	(N=415)	44	42	2
Teens not Pregnant at Baseline	(N=256)	49	59	-10
Teens in an AFDC Household at Baseline	(N=441)	44	52	- 8
Teens not in an AFDC Household at Baseline	(N=220)	47	44	3
Teens in School at Baseline	(N=373)	37	40	- 3
Teens not in School at Baseline	(N=298)	56	58	- 2
Teens With Work Experience at Baseline	(N=418)	43	50	- 7
Teens Without Work Experience at Baseline	(N=254)	47	45	2
Boston/Hartford Teens	(N=114)	43	59	-16
Harlem/Bedford Stuyvesant Teens	(N=185)	35	44	- 9
Phoenix/San Antonio Teens	(N=287)	50	50	0
Riverside/Fresno Teens	(N= 85)	46	42	4
Sample I Teens	(N=383)	48	55	- 7
Sample II Teens	(N=288)	42	40	2
All Teens	(N=671)	45	49	- 4

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The percentages shown have been adjusted for number of months at risk to a subsequent pregnancy, pregnancy status at baseline, age at first birth, number of semesters in a teen parent school program at baseline, the time elapsed between baseline and the 24-month interview, the number of times dropped out of school at baseline, and experience with oral contraceptives at baseline.

+Statistically significant at the .10 level.

statistically significant except among Puerto Rican teens. For this subgroup, nearly 50% more of the comparison than the experimental teens had had a repeat pregnancy 24 months after entering the study. In several other subgroups (older teens, teens who entered as mothers and teens living in an AFDC household at baseline) the experimental advantage just missed being significant at the .10 level for a two-tailed test. Clearly though, by the time of the 24-month interview, many of the experimental teens had "caught up" with comparison teens in terms of having a subsequent pregnancy.

By the final interview, one fourth of the study sample had had a second or higher-order live birth since baseline. For the sample as a whole, experimental teens (22%) had significantly fewer subsequent live births by the time of the final interview than comparison group teens (29%). An experimental group advantage was observed for almost every subgroup, reaching levels of statistical significance for eight of the nineteen subgroup comparisons (Table 4.7). Again, the most sizable impact was observed among the Puerto Rican teens. In this subgroup, twice as many comparison as experimental teens had delivered a baby that was conceived subsequent to the index pregnancy. It should be noted that the difference in the percent of teens with a repeat pregnancy and the percent with a repeat live birth at the final interview is accounted for primarily by the fact that many teens with a subsequent pregnancy had not yet delivered by the time of the final interview (i.e., the group difference does not reflect a significantly higher rate of abortion among experimental teens). The experimental group advantage in the live birth rate at 24 months post-baseline is the logical extension of the fact that experimental teens had fewer pregnancies at 12 months post-baseline.

TABLE 4.7

ADJUSTED PERCENTAGES OF TEENS WITH A LIVE BIRTH SUBSEQUENT TO THE INDEX PREGNANCY AT 24-MONTH INTERVIEW, FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Adjusted Percentages, by Group		
		Experimental	Comparison	Difference
Black Teens	(N=310)	20	30	-10+
Mexican American Teens	(N=163)	26	29	- 3
Puerto Rican Teens	(N=119)	21	41	-20+
Teens Age 15 or Younger at Baseline	(N=223)	25	27	- 2
Teens Age 16-17 at Baseline	(N-448)	20	31	-11*
Teens Pregnant at Baseline	(N=415)	20	22	- 2
Teens not Pregnant at Baseline	(N=256)	26	42	-16*
Teens in an AFDC Household at Baseline	(N=441)	21	32	-11*
Teens not in an AFDC Household at Baseline	(N=230)	26	26	0
Teens in School at Baseline	(N=373)	14	20	- 6
Teens not in School at Baseline	(N=298)	32	41	- 9
Teens With Work Experience at Baseline	(N=418)	23	28	- 5
Teens Without Work Experience at Baseline	(N=254)	20	31	-11+
Boston/Hartford Teens	(N=114)	22	38	-16
Harlem/Bedford Stuyvesant Teens	(N=186)	10	24	-14*
Phoenix/San Antonio Teens	(N=287)	29	35	- 6
Riverside/Fresno Teens	(N= 85)	19	14	5
Sample I Teens	(N=383)	25	34	- 9+
Sample II Teens	(N=288)	21	23	- 2
All Teens	(N=671)	22	29	- 7+

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The percentages shown have been adjusted for number of months at risk to a subsequent pregnancy, pregnancy status at baseline, age at first birth, and the number of pregnancies at baseline.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.

Returning to Table 4.4, the teens in the experimental group had only a marginally smaller mean number of pregnancies (.1) at the conclusion of the study, despite the fact that the experimental teens had had fewer repeat pregnancies 12 months into the study. All these findings reflect the fact that experimental teens were able to postpone their subsequent pregnancies somewhat longer than comparison teens. The difference, though statistically significant, was not large: experimental teens waited an average of about two months longer than comparison teens. On average, those who became pregnant again did so within 14 months of delivering the index child. Interestingly, although teens in the experimental and comparison groups had a similar total number of pregnancies at the end of the study, teens who received birth control counseling from the program (but not from another source) had significantly fewer pregnancies than those who did not (see Table E.8 in Appendix E for the results of the regression analysis). However, as discussed in Chapter 2, this finding must be interpreted cautiously due to potential selection biases.

The second approach in studying pregnancy outcomes was to use as outcome variables the incidence of repeat pregnancy at fixed intervals after the termination of the index pregnancy. The results of these analyses are presented in Table 4.8. Because of the cohort problem mentioned earlier, two sets of estimates were calculated: (1) for all teens for whom postpartum rates could be calculated, resulting in smaller subsets of available cases as the interval increases (Panel A); and (2) for a constant cohort of teens for whom there were 24-month postpartum data, resulting in the exclusion of most teens who were pregnant at baseline (Panel B).

TABLE 4.8

ADJUSTED RATES OF SUBSEQUENT PREGNANCY AT 12, 18 AND 24 MONTHS POSTPARTUM[a], BY EXPERIMENTAL AND COMPARISON GROUP[b]

Pregnancy Outcome	Adjusted Mean or Percentage, by Group		
	Experimental	Comparison	Difference
A. Entire Sample[c]			
Percent With a Subsequent Pregnancy 12 Months Postpartum (N=673)	21	23	- 2
Percent With a Subsequent Pregnancy 18 Months Postpartum (N=589)	34	38	- 4
Percent With a Subsequent Pregnancy 24 Months Postpartum (N=345)	47	52	- 5
B. Sample for Whom 24-Month Post-Partum Data Were Available[c]			
Percent With a Subsequent Pregnancy 12 Months Postpartum (N=345)	15	25	-10**
Percent With a Subsequent Pregnancy 18 Months Postpartum (N=345)	30	39	- 9*
Percent With a Subsequent Pregnancy 24 Months Postpartum (N=345)	47	52	- 5

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Postpartum refers to an index pregnancy. The index pregnancy was either the pregnancy in progress at the time of the baseline interview (pregnant teens) or the most recently terminated pregnancy at baseline (teen mothers).

[b] The percentages in this table have been adjusted for ethnicity, school status at baseline, pregnancy status at baseline, number of baseline pregnancies, number of semesters in a teen parent program at baseline, and number of months between baseline and termination of the index pregnancy. The full regression tables are presented in Table E.10 to E.12 of Appendix E.

[c] Panel A presents data for every teen for whom repeat pregnancy information was available at the specified interval. Since many teens were pregnant at baseline, 24 months following the pregnancy had not yet elapsed at the time of the final interview for these teens. Panel B presents data for a constant cohort for whom data were available at all three intervals.

*Statistically significant at the .05 level.
**Statistically significant at the .01 level.

Panel A of Table 4.8 indicates that, for all teens for whom data were available, the experimental teens had slightly lower rates of repeat pregnancy at all three time intervals, but none of these differences was statistically significant. Panel B indicates that among the teens for whom 24-month postpartum data were available, the experimental teens had substantially lower rates of repeat pregnancy at 12 months postpartum, and that group differences diminished over time. Since the Panel B data are almost exclusively from teens who entered the study as teen mothers, these results are consistent with subgroup results presented in Tables 4.5 and 4.6, which indicated that the program had substantially greater short-term success in the area of fertility outcomes with teens who entered the program as mothers than with those who entered pregnant.

In summary, it appears that during the first year after enrolling in Project Redirection, experimental teens had lower rates of repeat pregnancy than comparison teens. In certain subgroups, such as black teens and teen mothers, the difference was of a sizable magnitude. However, in the last 12 months of the study, by which time most experimental teens were no longer in the program, the two groups had similar rates of repeat pregnancy, except in the subgroup of Puerto Rican teens. The two groups had an identical mean number of pregnancies at the end of the study, but the experimental teens took a modestly longer amount of time to achieve their post-baseline pregnancies. The next section examines the contraceptive behaviors of these teens.

C. Contraceptive Utilization: An Overview

According to data from a national survey of young women aged 15 to 19 in 1979, 73% of black teens and 44% of white teens have had premarital intercourse by age 17 (Zelnik and Kantner, 1980). The majority of sexually active teens, however, run the risk of a premarital pregnancy by failing to consistently use effective contraception, despite the fact that most do not intend to become pregnant. According to self-reports from the national survey, only about a third of the sexually active teens said they always used contraception; 36% of black teens and 27% of white teens admitted that they had never used contraception. Overall, sexually active teens reported a delay of about 12 months between first intercourse and the time of obtaining a medically prescribed contraceptive (Zelnik et al., 1984). Among those teens who had a premarital pregnancy, only about one in five said the pregnancy was wanted (Zelnik and Kantner, 1980).

Given the emerging concerns about the problem of teenage pregnancy, considerable research attention has focused on why teenagers are not more successful in avoiding an unintended pregnancy. It appears that knowledge of and access to methods of birth control are not the prime determinants of contraceptive use. While few teenagers have detailed or technical information about specific contraceptives, almost all teenagers know of the existence of methods to avoid pregnancy, and most know, in general, how contraceptives could be obtained (Shah et al., 1975; Zelnik and Kantner, 1979).

In the national survey, when teenagers who were not consistent contraceptors were asked directly why they did not use contraception, the most common response was that they did not think they could become pregnant, either because of their age, the infrequency of intercourse, or the time of the month. Other commonly cited reasons were that intercourse had not been expected (20%), that circumstances made it difficult (7%), or that they felt birth control was wrong or dangerous (5%). Nearly 10% of the black teens, but 3% of the white teens, said that contraceptives were either too difficult to use or interfered with sexual pleasure (Zelnik and Kantner, 1979).

Since self-reports of reasons for non-use may be misleading, researchers have more often explored factors that are correlated with contraceptive utilization among teens. Teens who are most likely to expose themselves to the risk of an unintended pregnancy are more likely to be young (Zelnik and Kantner, 1980; Hammerslough, 1984; Furstenberg et al., 1983; Foreit and Foreit, 1978); black (Zelnik and Kantner, 1980; Burnett et al., 1980; Hammerslough, 1984; Furstenberg et al., 1983); and poor (Shah et al., 1975; Hornick et al., 1979; Furstenberg et al., 1983; Hammerslough, 1984). In short, although the dynamics of contraceptive use among sexually active teenagers remains poorly understood, there is considerable evidence that the teens in the present study represent a group whose contraceptive behavior is especially likely to be inadequate.

Table 4.9 presents some information concerning the study sample's sexual and contraceptive behavior over the two-year study period. This table indicates that, for most teens in the sample, sexual intercourse was neither sporadic nor infrequent. At all three interviews the majority reported having had sex within the past three months. At the 24-month interview, 53% said they had had intercourse within the previous two weeks (not shown in table). Of those teens who were sexually active, the majority reported at all three interviews that they had sex once or more each week.

At baseline, only about two-fifths of the sample had any experience with contraceptives, but this percentage doubled by the time of the final interview. Experience with all major forms of birth control rose sharply

TABLE 4.9

SELECTED SEXUALITY AND CONTRACEPTIVE VARIABLES FOR THE IMPACT ANALYSIS SAMPLE AT BASELINE, 12-MONTH AND 24-MONTH INTERVIEWS

Selected Variable	Baseline Interview		12-Month Interview		24-Month Interview	
	Sample I Teens	All Teens	Sample I Teens	All Teens	Sample I Teens	All Teens
Percent Having Had Sexual Intercourse in Past 3 Months	59.7	—	76.2	76.6	78.4	77.9
Percent of Sexually Active Teens Having Sex at Least Once a Week	72.1	—	58.0	60.6	69.3	69.4
Percent Ever Used Any Method of Contraception		43.8		79.4		88.7
Percent Ever Used Oral Contraceptives		28.4		69.6		82.2
Percent Ever Used an IUD		3.1		12.7		21.8
Percent Ever Used a Diaphragm		2.5		8.3		12.1
Percent Whose Partner Ever Used a Condom		12.1		31.1		44.3
Percent Used Contraception at Last Intercourse		—		67.0		53.8
Percent Saying Access to Oral Contraceptives is Easy	73.3	—	82.9	81.8	86.2	83.7
Mean Score, Birth Control Knowledge Test[a]	9.2	—	9.4	9.4	10.4	10.3

SOURCE: Tabulations are from AIR interviews with expermental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: When baseline information could be obtained retrospectively for Sample II respondents, the information shown is for the entire impact analysis sample (N=675). When baseline information was available only for Sample I respondents, baseline figures are shown only for those respondents (N=383) and follow-up information is shown both for Sample I separately so that trends could be evaluated, and for the aggregated impact analysis sample.

[a] The Birth Control Knowledge Test was a 16-item test designed to measure knowledge about various contraceptive methods and risk of pregnancy. Scores could range from 0 (no correct answers) to 16 (all correct answers).

between baseline and the 12-month interview, and continued to rise to the 24-month interview. The percentage of teens having ever used an IUD nearly doubled in the last 12 months of the study, suggesting the possibility that many teens adopted this method after a higher-order birth. Despite the teens' extensive first-hand experience with birth control methods at the end of the study, about half the sample admitted that they had failed to protect themselves against another pregnancy at their last intercourse.

Table 4.9 also suggests that access to and knowledge of birth control are probably not the main obstacles to contraceptive use. Even at baseline, when only 44% said they had ever used birth control, nearly three-fourths said access to the pill would be easy. Furthermore, the teens got an average of 9.2 correct answers on a 16-item test of birth control. Since nearly 90% said they had used contraceptives at least once at the final interview, the fact that only about half were protected at last intercourse cannot be attributed to ignorance of how to avoid a pregnancy.

Ethnic group differences with respect to sexual and contraceptive behaviors tended to be substantial, as shown in Table 4.10. Black teens had the highest rate of recent sexual contact, but the lowest rate of frequent activity; for Puerto Ricans this pattern was reversed.

The majority of teens in all ethnic groups had used contraceptives at least once, but black teens had the lowest rate of contraceptive use, while Puerto Ricans had the highest. Experience with different contraceptive methods also varied by ethnicity. While the ethnic groups were comparable with respect to use of the pill, Puerto Ricans were substantially more likely than others to have used an IUD, but less likely

TABLE 4.10

SELECTED SEXUALITY AND CONTRACEPTIVE VARIABLES REPORTED AT 24-MONTH INTERVIEW, BY ETHNICITY

Selected Variable	Black Teens	Mexican American Teens	Puerto Rican Teens	White Teens	All Teens[a]
Percent Sexually Active Within Previous 3 Months	81.7	78.4	66.1	79.3	77.9*
Percent of Sexually Active Teens Having Sex At Least Once a Week[b]	60.2	66.4	92.4	87.0	69.4***
Percent Ever Used Contraceptive	85.3	89.6	95.0	93.2	88.7*
Percent Ever Used Oral Contraceptive	80.2	84.1	85.7	84.7	82.2
Percent Ever Used IUD	19.8	19.5	31.9	13.6	21.8**
Percent Whose Partner Ever Used Condom	49.8	40.9	26.9	61.0	44.3***
Percent Ever Used a Diaphragm	16.0	4.9	7.6	15.3	12.1***
Percent Who Used Contraception at Last Intercourse	49.5	57.3	55.5	64.4	53.8
Mean "Riskiness" of Last Contraceptive Method Used[c]	41.0	36.1	41.4	33.1	39.2
Mean Score on Birth Control Knowledge Test[d]	10.9	10.0	8.3	12.3	10.3***

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at 24 months after baseline.

NOTES: [a] The last column presents information for all teens in the sample, including 20 teens whose ethnicity was not black, Mexican American, Puerto Rican, or white.

[b] The number of sexually active teens at the 24-month interview was 592.

[c] The "riskiness" of a method was the estimated percentage of women who would become pregnant in one year, using the specified method. The higher the number, the higher the risk of pregnancy. For example, oral contraceptives were coded 4; use of no method was coded 90.

[d] The Birth Control Knowledge Test was a 16-item test designed to measure knowledge about various contraceptive methods and risk of pregnancy. Scores could range from 0 (no correct answers) to 16 (all correct answers).

*The group difference is statistically significant at the .05 level.

**The group difference is statistically significant at the .01 level.

***The group difference is statistically significant at the .001 level.

to have used condoms. White teens had an especially high rate of experience with condoms. Despite these different patterns of experience, teens in the four ethnic groups had comparable percentages who used birth control at last intercourse, and who used comparably effective methods.

When teens' reasons for not using contraception were explored during the final interview, the most prevalent response among those who had never used any method was that they were concerned about the adverse side effects of birth control (30%). Ten percent said it was too much trouble, and another 9% said they just did not like birth control. Teens with inconsistent contraceptive use were asked why they did not always protect themselves. The most common responses were the following: concern about harmful side effects (34%), failure to remember to take pills daily (13%), dislike of birth control (9%), and infrequent sexual activity (6%). Teens who used a method at last intercourse reported high levels of satisfaction with medically prescribed methods (pill--87%; IUD--85%; and diaphragm--86%), but somewhat less satisfaction with other methods (e.g. condoms--42%; withdrawal--67%; spermicides--50%).

In summary, teens in the study sample gained considerable experience with contraception during the two-year study period. Their use of medically-prescribed methods was higher than that reported for national samples of sexually active teenage women (Zelnik and Kantner, 1980). Nevertheless, consistent with the fact that many had had an early repeat pregnancy, regular use was infrequent. Although commentators have often claimed that one potential explanation for teens' inconsistent use of contraceptives is their sporadic or infrequent sexual activity, this rationale cannot be applied to the respondents in this study, most of whom had regular and frequent intercourse.

D. Redirection Impacts on Contraception

Birth control counseling and information was a key service component in Project Redirection. As noted in Chapter 3, a majority of teens in both the experimental and comparison groups did acknowledge having had birth control counseling. In addition to direct discussions regarding contraception, Project Redirection attempted to motivate teens to avoid another pregnancy through encouragement for further schooling and employment. This section examines the program's success regarding the teens' contraceptive use.

Table 4.11 summarizes information relating to contraceptive use and knowledge in the experimental and comparison groups, after adjusting for baseline characteristics. At the time of the 12-month interview, a significantly higher percentage of experimental teens (78%) had used a medically prescribed method of birth control (the pill, IUD, or diaphragm) than comparison teens (69%). By the 24-month interview, however, the difference was small and nonsignificant, owing largely to the fact that substantially more comparison teens gained experience with these methods during the last year of the study. However, teens who reported having had birth control counseling--from any source--were significantly more likely to report experience with a medically prescribed contraceptive than those who did not.

In addition to asking respondents if they had ever used various methods of contraception, information was also obtained regarding contraceptive behavior at their last intercourse. At the 12-month interview, significantly more of the experimental (54%) than the comparison (45%) teens said they had used birth control at last intercourse. At the 24-month interview, the two groups no longer differed,

TABLE 4.11

SUMMARY OF ADJUSTED CONTRACEPTIVE OUTCOMES, BY EXPERIMENTAL AND COMPARISON GROUP[a]

Contraceptive Outcome	Adjusted Mean or Percentage, by Group		
	Experimental	Comparison	Difference
Percent Ever Used a Medically Prescribed Contraceptive, 12 Months Post-Baseline[b]	78	69	9*
Percent Ever Used a Medically Prescribed Contraceptive, 24 Months Postpartum	81	79	2
Percent Having Used Any Contraceptive at Last Intercourse, 12-Month Interview	54	45	9*
Percent Having Used Any Contraceptive at Last Intercourse, 24-Month Interview	54	54	0
Mean "Riskiness" of Contraceptive Used at Last Intercourse, 12-Month Interview[c]	29.90	32.68	-2.78
Mean "Riskiness" of Contraceptive Used at Last Intercourse, 24-Month Interview	34.53	35.64	-1.11
Mean Score, Birth Control Knowledge Test, 12 Months Post-Baseline[d]	9.44	9.35	.09
Mean Score, Birth Control Knowledge Test, 24 Months Post-Baseline	10.79	10.22	.55*
Number of Respondents	305	370	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] The means and percentages in this table have all been adjusted for ethnicity, school status at baseline, pregnancy status at baseline, and number of baseline pregnancies. Various other characteristics were also controlled, but different covariates were required for different outcomes. The full regression tables are presented in Tables E.13 and E.14 of Appendix E for all 24-month outcomes. All covariates significantly related to the outcomes, as shown in these appendix tables, were controlled in deriving the figures presented in this table. See also Footnote 2 at the end of this chapter.

[b] Medically prescribed methods include oral contraceptives, the IUD, and the diaphragm.

[c] The "riskiness" of a method was the estimated percentage of women who would become pregnant in one year, using the specified method. The higher the number, the higher the risk of pregnancy. For example, oral contraceptives were coded 4; use of no method was coded 90. This analysis was based on those teens who were not pregnant at the time of the follow-up interviews.

[d] The Birth Control Knowledge Test was a 16-item test designed to measure knowledge about various contraceptive methods and risk of pregnancy. Scores could range from 0 (no correct answers) to 16 (all correct answers).

*Statistially significant at the .05 level.

accounted for by the fact that the rate of birth control use rose for comparison but not experimental teens between the two interviews.

It was also possible to compare the groups with respect to the average effectiveness of birth control used at last intercourse. Teens were asked what method, if any, had been used. Based on data from large-scale contraceptive studies, each method was assigned a "risk" level (Hatcher *et al.*, 1980). The risk level associated with a contraceptive method is the number of women who would become pregnant in one year out of 100 using that method. For example, four sexually active women out of 100 would be expected to conceive in one year using the pill; ten using condoms; 17 using diaphrams; five using the IUD; and 90 using no method. Based on these ratings a "riskiness" score was assigned to each non-pregnant teen according to the method last used. At both the 12- and 24-month interviews, experimental teens had lower riskiness scores than comparison teens. Since at 24 months post baseline identical percentages in both groups had used a method at last intercourse, this means that experimental teens were using somewhat more effective methods. However, the group differences for the riskiness scores were not statistically significant at either interview.

Teens were also administered a 16-item test of birth control knowledge at all three interviews. After controlling baseline test scores and other covariates, Sample I teens in the experimental group scored higher than teens in the comparison group at both interviews, though the difference was significant only at the later interview.[3] A further analysis (shown in Table E.14 of Appendix E) revealed that teens who received birth control counseling from Project Redirection (or from another source) were especially likely to score well on the birth control knowledge test at 24 months.

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[3] Sample II experimental teens also scored significantly higher than comparison teens on the Birth Control Knowledge Test, after adjusting for background characteristics. However, since it was not possible to control baseline test scores for Sample II, this finding should be treated cautiously.

In summary, participation in Project Redirection resulted in some long-term knowledge gains in the area of contraception. However, the short-term behavior gains in this area disappeared by the time of the final interview. In both groups, teens continued to expose themselves to the risk of another pregnancy. Among those teens not pregnant at the 24-month interview, the average "riskiness" of the contraceptive method used at last intercourse was about 35. Assuming that these teens' contraceptive patterns remain relatively constant, this analysis suggests that approximately 35 percent of the teens not pregnant at the 24-month interview might conceive within 12 months of the final interview.

E. Health Outcomes

There is widely documented evidence that young parents and their offspring are at greater risk to a variety of health problems than older mothers and their infants. One of the most serious medical problems associated with teenage pregnancy is the increased risk of prematurity and low birth weight babies (Broman, 1978; Graham, 1981; Menken, 1975; Stickle, 1981; Taffell, 1980). Furthermore, the rate of low birth weight infants (infants under 5 1/2 pounds at birth) is more than twice as high for blacks (13%) as for whites (6%) (National Center for Health Statistics, 1982). The combined factors of youth, poverty and minority status place the target population for Project Redirection at particularly high risk to prematurity and other fetal and neonatal health problems.

National data from 1980 birth records indicate that 14% of nonwhite teens age 17 or under had a low birth weight baby (NCHS, 1982).

Nutritional factors and late or inadequate prenatal care have been suggested as contributing causes to prematurity and other medical problems (Bonham and Plaček, 1978; Forbes, 1981; Frisancho _et al._, 1983; Menken, 1975; Carruth, 1978). National data, in fact, have shown that young women receive less prenatal care than older pregnant women. For example, a report from the National Center for Health Statistics (1982) indicated that only 54 percent of teens age 15 to 19 obtained prenatal care in the first trimester, compared with 74 percent of all women. Among teens under 15 years, the rate was only 33%.

Since low birth weight is a contributing factor in a number of long-term medical problems (cerebral palsy, mental retardation, epilepsy, and other neurological defects), a major objective of many programs for pregnant teenagers is the delivery of prenatal health care early in the pregnancy. There is, in fact, evidence that these efforts have had some success. Several evaluation reports indicate that program participants have a lower rate of low birth weight infants than either a control group or the national figures (Burt _et al._, 1984; Dickens _et al._, 1973; Chanis, 1979; Flick, 1983; Klerman and Jekel, 1973; Knapp and Drucker, 1973; McAnarney _et al._, 1978; Osofsky, 1970). Findings from several of these studies are presented in Appendix D.

As reported in Chapter 3, almost all of the subjects in this study had received medical services during the two-year study period. Even at baseline, the vast majority of teens reported having received health-related services for themselves and their infants within the previous three months. Nearly three out of four of the teens pregnant at the baseline interview reported having received medical care during the first three months of their pregnancy. The majority of mothers (82 percent) reported postpartum care within ten weeks of delivery. Few teens reported major long-term problems for either themselves or their infants. Nevertheless, 15% percent of the babies born to these young mothers weighed under 5 1/2 pounds at birth, which is just above the national rate for nonwhite young teens.

In the 12-month follow-up interview, teens pregnant at baseline were asked about the outcomes of their pregnancies. Although the data are limited and were gathered by self-report, without verification by medical reports, they have interest as general indicators of the sample's health.

Almost all (93%) of the 390 teens who were pregnant at baseline reported at follow-up that they had visited a doctor five or more times for prenatal care. Nearly 75% reported eight or more prenatal visits. The amount of prenatal care was similar for experimental and comparison group teens. There were no significant age or ethnic group differences.

These teens had spent an average of 3.6 days in the hospital during and after delivery, which is comparable to the 3.9 days reported by teen mothers at baseline. White teens had a somewhat shorter mean length of stay (3.1 days) than other teens, but experimental-comparison, ethnic, and age group differences were not statistically significant.

The babies born to teens who were initially pregnant weighed, on average, 6.9 pounds at birth. Only 7% of the babies weighed under 5 1/2 pounds, which is a substantially lower percentage than that reported by the teens who were mothers at baseline, and lower also than national percentages. The mean birth weight of infants born to experimental and comparison teens was 6.8 pounds and 7.0 pounds, respectively. This difference was not significant.[4] White infants weighed 7.5 pounds, on average, compared to 6.7 pounds for black infants, 6.8 for babies of Puerto Rican descent, and 7.1 for babies of Mexican American descent ($p < .01$). The birth weight of the infants was not related to maternal age.

In summary, the follow-up data for teens pregnant at baseline suggested that most teens obtained extensive prenatal care and that the rate of low birth weight infants was lower than national norms in both the experimental and comparison groups. Since comparison teens were also a highly served group, these findings add to the growing body of literature (discussed in Appendix D) that suggest that special services to pregnant teenagers yield improved health outcomes to their infants.

F. Child Care

Without some form of child care, participation in school, employment workshops, or other activities of a teen parent program would not be possible. One of the components of Project Redirection was the brokering of child care arrangements for those who needed it. This section examines child care for the teens in the research sample.

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[4] To further test group differences in relation to birth weight, a regression analysis was performed in which infant's birth weight was regressed on various background factors and participation in the Redirection program. Included in the regression model were such variables as amount and timing of prenatal care, baseline AFDC status, age, ethnicity, baseline marital status, parental education, and number of prior pregnancies. The overall relation of these covariates and the program participation variable with the infant's birthweight was negligible: the highest adjusted R^2 obtained was .01, and the overall model was not statistically significant. (Similar results were reported in Burt's (1984) study of OAPP clients.) Thus, it was concluded that there was no experimental/comparison difference in birth weight after controlling key background variables.

Information on the teens' child care arrangements was obtained at all three interviews. On each occasion, teens in both the experimental and comparison groups reported that the maternal grandmother was the single most important provider of child care. Child care arrangements as reported in the final interview are show in Table 4.12. Relatives accounted for about half of all the child care arrangements in both groups. More than twice as many experimental teens as comparison teens had put their child in a day care center while at school or work, but this difference was not significant. Over the two-year study period there were few sizable changes in how these teens arranged for the care of their babies while they worked or went to school.

The majority of teens (86% in both groups) reported at the final interview that their current arrangements met their child care needs, and similar percentages said they had no problems with their existing method. Among those who expressed some dissatisfaction, nearly half (43%) were concerned about the unreliability of the care. Another 28% said no babysitter was available.

Respondents were also asked if they ever missed school or work because of a child care problem. Thirty percent of the experimental teens and 34% of the comparison teens acknowledged that they had, but absences due to child care problems were reportedly infrequent.

Thus, in both groups, there was a strong reliance on relatives--especially the maternal grandmother--for child care assistance. Only about one-fourth of the teens used paid child care, and little change was reported in this regard over the two-year study period. By and large, the teens reported being satisfied with their current arrangements. There were no significant group differences with respect to child care.

TABLE 4.12

CHILD CARE ARRANGEMENT USED DURING SCHOOL OR WORK REPORTED AT 24-MONTH INTERVIEW, BY GROUP

Child Care Arrangement	Percentage Distribution, by Group Experimental	Comparison	Both Groups
Respondent Takes Child With Her	4.3	4.9	4.6
Child's Father	1.9	0.9	1.4
Maternal Grandmother	33.3	34.6	34.0
Other Relative	13.8	16.0	15.0
Friend or Neighbor (Unpaid)	3.8	1.3	2.5
Paid Babysitter	13.3	12.0	12.6
Day Care Center	12.9	5.8	9.2
Combination of Above/Other	16.7	24.4	20.7
Total	100.0	100.0	100.0

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I and II at 24 months after baseline.

NOTES: The totals may not add to 100.0 percent due to rounding error.

The responses for this table were provided only by the 435 teens who had either been in school or employed within the previous 12 months.

The group differences were not statistically significant at the .05 level.

CHAPTER 5

PROGRAM IMPACTS ON EDUCATIONAL OUTCOMES

Educational attainment is a prime determinant of lifelong income, occupational status, and economic self-sufficiency. Those who fail to complete basic schooling--a high school diploma or GED--are particularly disadvantaged in the marketplace. Unemployment data suggest that a female high school dropout is even more handicapped than a male dropout. In 1979, the unemployment rate for female dropouts was 37%, while for male dropouts the rate was 19%; both were nearly double the rate for high school graduates (U.S. Department of Labor, 1981). In an economy that has declined since 1979, those without a diploma have faced even stiffer competition for scarce entry-level jobs.

There is considerable evidence that early childbearing is related to curtailed schooling. For example, the Alan Guttmacher Institute (1981), in a special document on teenage pregnancy, has reported that by age 18, 60% of all teen mothers have not completed high school, and by age 19, the rate is still 40%. Mott and Maxwell (1981), using data from a national survey, found that the educational deficits of school-age mothers declined from 1968 to 1979, but McCarthy and Radish (1982) have suggested that their disadvantage relative to later childbearers has remained constant.

In any event, it is clear that large deficits still remain. For example, nine months after delivery, only 17% of white school-age mothers and 39% of black school-age mothers were in school in 1979 (Mott and Maxwell, 1981). The diminished lifelong educational attainment of adolescent parents has also been found to persist even when socioeconomic and motivational factors are controlled (Card and Wise, 1978; Moore and Waite, 1977). Furthermore, a sophisticated analysis that examined the direction of causality between childbearing age and education provided evidence that a birth during the high school years affects the teen's educational experiences and not vice versa (Moore and Burt, 1982). Further information from the literature on the educational experiences of teen mothers is presented in Appendix D.

Like many other teen parent programs, Project Redirection placed a high priority on encouraging its clients to complete school. Since the Redirection programs did not generally offer schooling directly, the educational component consisted of coordination with educational alternatives, referrals and placements, educational counseling, and tutoring for teens with special needs. It seems likely that, at least with respect to coordination and administrative support, Project Redirection offered some features not generally found in educational programs for teen parents (see, for example, Zellman, 1982). As reported in Chapter 3, teens in the experimental group reported having received educational counseling on more than five occasions, on average. A substantial number felt that the educational component was the most helpful aspect of the program. This chapter further examines the educational experiences of the teens in this study.

A. Educational Behaviors and Attitudes: An Overview

Table 5.1 summarizes trends over the two-year study period with respect to several educational outcomes for the aggregated study sample. Despite the fact that many teens were 16 years of age or younger at baseline, only slightly more than half were in an educational program. By 12 months after baseline, the percent of teens in school (or completed) remained fairly stable. However, by 24 months after baseline, fewer than half the sample had a positive school status. By the end of the study one out of five teens had completed their basic schooling and about 7% were obtaining postsecondary education.

For the sample as a whole, educational deficits were disturbingly high. At baseline, when these teens should have been, on average, in the 10th to 11th grade, the mean highest grade completed was 8.8. Two years later, when these teens should have been completing high school, the mean highest grade completed was 9.8. By the end of the study, most teens had dropped out of school at least once and nearly 40% had dropped out two or more times. Studies have shown that being behind grade for age is a major factor in failing to complete school (Rumberger, 1981).

At baseline almost all of the teens who were not in school said they had plans to return. For many teens this expectation was not realized and the expectation declined over time. By the end of the study, only about a half of the dropouts planned to return to school. Despite this fact and despite their educational deficits, about half the sample said at each interview that they wanted more schooling than a diploma or GED certificate, a goal which, for many, may be unrealistic without considerable "redirection." Other investigators have similarly reported a

TABLE 5.1

SELECTED EDUCATIONAL VARIABLES FOR THE IMPACT ANALYSIS SAMPLE AT BASELINE, 12-MONTH AND 24-MONTH INTERVIEWS

Selected Variable	Baseline Interview		12-Month Interview		24-Month Interview	
	Sample I Teens	All Teens	Sample I Teens	All Teens	Sample I Teens	All Teens
Percent in School or a GED Program		55.7		45.5		28.7
Percent Completed School/GED		0.0		7.9		19.6
Percent With Any Post-secondary Schooling		0.0		1.9		6.7
Mean Highest Grade Completed		8.8		9.2		9.8
Percent of Those not In School Planning To Return	81.1	——	59.5	59.6	52.4	54.7
Percent Wanting More Education Than a Diploma/GED	51.4	——	42.3	39.6	52.6	50.7
Percent Ever Dropped Out of School		54.1		69.6		79.1
Percent Dropped Out of School 2 or More Times		10.2		24.0		37.5

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: When baseline information could be obtained retrospectively for Sample II respondents, the information shown is for the entire impact analysis sample (N=675). When baseline information was available only for Sample I respondents, baseline figures are shown only for Sample I (N=383) and follow-up information is shown both for Sample I and the aggregated impact analysis sample separately.

tendency for low-income teens to state educational aspirations that are unrealistically high, given their educational status or programs (e.g. Tittle, 1981; Kenkel, 1984).

Ethnic group differences in the educational arena were substantial, as shown in Table 5.2. At the final interview, nearly four times as many blacks as Puerto Ricans were in school. Blacks were also more likely to be in school than whites, consistent with data from other studies of teen parents (Hofferth and Moore, 1979; Howell and Frese, 1982; Mott and Maxwell, 1981; Testa, 1983). However, whites were substantially more likely than other teens to have obtained their diplomas or GED certificates. There were no significant ethnic group differences with respect to postsecondary schooling.

During the two-year study period, the majority of teens had spent at least one entire semester in a school program. Hispanic teens, however, were underrepresented in this regard. Over 80% of blacks, but only 45% of Puerto Rican teens were enrolled one or more semesters post-baseline. Given this fact, it is not surprising that Puerto Rican teens had only completed 9.0 years of schooling on average at follow-up, and had the highest rate of multiple dropouts of any ethnic group.

Ethnic differences were also observed with regard to educational plans and aspirations. Here again it appeared that Hispanic teens were more disadvantaged than others. Fewer Puerto Rican or Mexican American teens than black or white teens reported wanting more than a high school diploma. Particularly noteworthy, however, is the fact that non-Hispanic teens aspired to and expected similar amounts of schooling, while among Hispanic teens there was a large discrepancy. More than twice as many Puerto Rican teens said they wanted postsecondary schooling as said they

TABLE 5.2

SELECTED EDUCATIONAL OUTCOMES AT 24-MONTH INTERVIEW, BY ETHNICITY

Selected Educational Outcome	Black Teens	Mexican American Teens	Puerto Rican Teens	White Teens	All Teens[a]
Percent Enrolled in School/GED Program	42.2	17.1	11.8	27.1	28.7***
Percent Completed School or GED	20.1	16.5	10.9	42.4	19.6***
Percent With Some Post-secondary Schooling	7.7	6.1	4.2	8.5	6.7
Percent in School One or More Semesters Post Baseline	80.8	59.1	44.5	78.0	68.6***
Mean Highest Grade Completed	10.2	9.3	9.0	10.6	9.8***
Percent Wanting More Education Than a Diploma/GED	64.0	48.1	52.6	64.3	50.7***
Percent Expecting More Education Than a Diploma/GED	64.0	30.0	23.9	54.9	47.8***
Percent of Those Not in School Planning to Return to School[b]	73.0	47.4	31.1	61.0	54.7***
Percent Dropped Out of School Two or More Times	34.1	38.4	42.9	33.9	37.0*
Percent Ever in Teen Parent School Program	52.7	47.6	34.5	59.3	48.7**
Number of Respondents	313	164	119	59	675

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at 24 months after baseline.

NOTES: [a]The last column presents information for all teens in the sample, including 20 teens whose ethnicity was not black, Mexican, Puerto Rican, or white.

[b]The number of teens not in school at the 24-month interview was 468.

*The group difference is statistically significant at the .05 level.

**The group difference is statistically significant at the .01 level.

***The group difference is statistically significant at the .001 level.

expected to get it (53% versus 24%). This difference could reflect the Puerto Rican teens' more accurate assessment of their future schooling or a greater sense of hopelessness in achieving their goals. In any event, substantially fewer Puerto Rican teens than other teens who were not in school when re-interviewed said they had specific plans to return in the near future.

Table 5.3 compares the four major ethnic groups with respect to school status at fixed intervals after termination of the index pregnancy. The picture that emerges is consistent with the data in the previous table. At 12, 18, and 24 months postpartum, Puerto Ricans were least likely to be in school, followed by the Mexican American teens. Overall, the percentage of teens in school remained fairly stable from 12 to 24 months postpartum, with about 40% to 45% enrolled or completed at all three time periods. However, the stability should not be interpreted to mean that the same 40-45% remained in school, since dropping out and returning to school was the typical pattern for these teens.

In summary, data obtained in the final interview suggest that many of these teens were even farther behind grade for age at the end of the study than at baseline. On average, the highest grade completed was under ten years for these teens who averaged just over 18 years of age. Puerto Rican teens had the most severe educational deficits of any ethnic group. The baseline interview revealed that some of the educational deficits of these subjects occurred prior to the initial interview. However, pregnancy and childrearing were cited as the major reasons for dropping out of school, consistent with national data on school-age women (AGI, 1981). Given the findings on repeat pregnancy cited in the previous chapter, the obstacles to eventually catching up with peers who delayed

TABLE 5.3

SCHOOL STATUS AT SELECTED INTERVALS SUBSEQUENT TO INDEX PREGNANCY, BY ETHNICITY

School Status Variables	Black Teens	Mexican American Teens	Puerto Rican Teens	White Teens	All Teens[a]
Percent in School/GED or Completed 12 Months Postpartum[b]	55.8 (310)[c]	37.8 (164)	27.1 (118)	51.7 (58)	44.9*** (670)
Percent in School/GED or Completed 18 Months Postpartum	49.8 (279)	30.6 (147)	26.0 (96)	62.6 (53)	41.6*** (594)
Percent in School/GED or Completed 24 Months Postpartum	50.6 (176)	35.1 (77)	25.4 (59)	61.8 (34)	44.1*** (358)

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] The last column presents information for all teens in the sample, including 20 teens whose ethnicity was not black, Mexican American, Puerto Rican, or white.

[b] Postpartum refers to the period following the index pregnancy. The index pregnancy was either the pregnancy in progress at the time of the baseline interview or the most recently terminated pregnancy at baseline.

[c] The number in parentheses indicates the number of teens about whom the information was available. Since some teens were pregnant at baseline, the school status of all teens 18 and 24 months postpartum could not be ascertained during the 24-month post-baseline interview.

***The group difference is statistically significant at the .001 level.

childbearing appear to be enormous. National data suggest that adolescent mothers do eventually compensate somewhat for their early educational losses, but that this compensation never eliminates their educational disadvantages (Card and Wise, 1978).

B. Redirection Impacts on Educational Outcomes

Because educational credentials are a major determinant of economic self-sufficiency, a central objective of Project Redirection was school continuation for its clients. This section examines whether participation in the program affected the experimental teens' educational behaviors during the two years of the study.

In analyzing educational differences between the experimental and comparison groups, controls for initial characteristics were crucial. As indicated in Chapter 2, the comparison group teens were substantially more likely than experimental teens to be in school at baseline. This difference could partially be the result of initial differences in parenting status (comparison teens were less likely to have already delivered), but could also reflect important differences in motivation, ability, perseverence, family support, and so on. Therefore, several covariates relating to the teens' baseline educational histories (school status at baseline, number of times dropped out of school, enrollment in a special teen parent program, and highest grade completed) were used in estimating the impact of participating in Project Redirection. The covariates chosen were ones suggested by the literature as having an effect on educational behaviors and which were identified in preliminary analyses as being correlated with outcome and/or participation measures.

The results are summarized in Table 5.4, and full regression tables are presented in Tables E.15 to E.18 of Appendix E. Twelve months after baseline, teens in the experimental group (56%) were significantly more likely than comparison group teens (49%) to either be in school or have their diplomas, after controlling initial differences. The seven percentage point difference means that nearly 15% more experimental than comparison teens had a positive school status at the 12-month interview. The program's impact was especially powerful among Hispanic teens, teens not in school at baseline, older teens, and teens who were pregnant at base- line (not shown in tables).

By the 24-month interview, the program effect had disappeared. Whereas 56% of the experimental teens had a positive school status at 12 months post-baseline, only 43% were in school or had completed school at 24 months post-baseline. The percentage declined in the comparison group as well, but the decline was relatively small (from 49% to 43%). Table 5.5 shows experimental and comparison group differences in 24-month school status for teens in various subgroups.[1] In some groups experimental teens had a better school status at the final interview, while in others the reverse was true. In only two subgroups, however, were the experimental and comparison group differences significant. Teens in the Boston Redirection program were significantly more likely than Hartford teens to have a positive school status at the 24-month interview. Also, among teens who had dropped out of school at baseline there were significant program impacts. Among teens who were dropouts at the beginning of the study, 28% of the experimental and 18% of the comparison

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[1] Subgroup results for some 12-month impacts are presented in tables at the end of Appendix E.

TABLE 5.4

SUMMARY OF ADJUSTED EDUCATIONAL OUTCOMES, BY EXPERIMENTAL AND COMPARISON GROUP[a]

Educational Outcome	Adjusted Mean or Percentage, by Group		
	Experimental	Comparison	Difference
Percent in School or Completed 12 Months Post-Baseline	56	49	7*
Percent in School or Completed 24 Months Post-Baseline	43	43	0
Percent in School or Completed 12 Months Postpartum[b]	51	41	10**
Percent in School or Completed 18 Months Postpartum	46	38	8*
Percent in School or Completed 24 Months Postpartum	45	43	2
Percent Received Diploma or GED Certificate 24 Months Postpartum	20	20	0
Percent Ever Enrolled in School or GED Program Baseline to 12-Month Interview	75	51	24***
Percent Ever Enrolled in School or GED Program, Baseline to 24-Month Interview	87	71	16***
Mean Number of Semesters Enrolled, Baseline to 12-Month Interview	0.90	0.46	0.44***
Mean Number of Semesters Enrolled, Baseline to 24-Month Interview	2.05	1.58	0.47**
Percent Aspiring to More Than a Diploma, 12-Month Interview	38	36	2
Percent Aspiring to More Than a Diploma, 24-Month Interview	45	51	-6

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]The means and percentages in this table have all been adjusted for ethnicity, school status at baseline, baseline participation in a teen parent program, and pregnancy status at baseline. Various other characteristics were also controlled, but different covariates were required for different outcomes. The full regression tables are presented in Tables E.12 to E.18 of Appendix E for all 24-month outcomes. All significant explanatory variables were controlled in deriving the figures presented in this table.

[b]Postpartum refers to the period following the index pregnancy. The index pregnancy was either the pregnancy in progress at the time of the baseline interview or the most recently terminated pregnancy at baseline.

*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE 5.5

ADJUSTED PERCENTAGES OF TEENS IN SCHOOL OR A GED PROGRAM OR COMPLETED SCHOOL AT 24-MONTH INTERVIEW, FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Adjusted Percentages, by Group		
		Experimental	Comparison	Difference
Black Teens	(N=313)	55	55	0
Mexican American Teens	(N=163)	27	34	- 7
Puerto Rican Teens	(N=119)	27	15	12
Teens Age 15 or Younger at Baseline	(N=224)	44	46	- 2
Teens Age 16-17 at Baseline	(N=450)	43	43	0
Teens Pregnant at Baseline	(N=416)	43	45	- 2
Teens not Pregnant at Baseline	(N=258)	42	42	0
Teens in an AFDC Household at Baseline	(N=428)	40	42	- 2
Teens not in an AFDC Household at Baseline	(N=230)	50	47	3
Teens in School at Baseline	(N=375)	53	62	- 9
Teens not in School at Baseline	(N=299)	28	18	10*
Boston/Hartford Teens	(N=114)	30	14	16*
Harlem/Bedford Stuyvesant Teens	(N=186)	55	51	4
Phoenix/San Antonio Teens	(N=289)	42	48	- 6
Riverside/Fresno Teens	(N= 85)	43	54	-11
Sample I Teens	(N=385)	46	44	2
Sample II Teens	(N=289)	41	43	- 2
All Teens	(N=674)	43	43	0

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The percentages have been adjusted for ethnicity, school status at baseline, highest grade completed at baseline, number of times dropped out of school at baseline, enrollment in a teen parent program at baseline, number of baseline pregnancies, and age at first birth.

*Statistically significant at the .05 level.

teens had a positive school status at the end of the study, an incremental improvement of 55%. This finding is consistent with the findings observed after 12 months (Polit et al., 1983), which suggested the program had the biggest impact with the most disadvantaged participants.

Returning to Table 5.4, it can be seen that one-fifth of the teens had completed their basic schooling by the 24-month interview. After adjusting for initial differences, the percentage completing school in the two groups was identical. Table 5.6 shows that group differences were generally small for all subgroups. The only significant difference was for baseline school dropouts. Among those not in school at baseline, 82% more experimental teens than comparison teens had completed school by the end of the study (20% versus 11%). Participating in Project Redirection appears to have removed the disadvantage generally associated with being a dropout: in the experimental group, teens were as likely to complete school whether or not they were in school at baseline. In the comparison group, fewer than half of the baseline dropouts had completed school within the two-year study period compared with those who were initially in school (11% versus 24%).

Table 5.4 also presents information on school enrollment over the 24 month post-baseline period. Within 12 months of the baseline period, 75% of the experimentals but only 51% of the comparison teens had ever been enrolled in an educational program since the baseline interview. This difference was highly significant. By 24 months, the experimental rate of school enrollment was up to 87%, but the comparison rate had also increased to 71%. The difference remained highly significant. Table 5.7 shows that the program's success in getting participants to return to or stay in school held up in virtually every subgroup. As in previous analyses, the effects were especially pronounced for teens not in school

TABLE 5.6

ADJUSTED PERCENTAGES OF TEENS HAVING RECEIVED A HIGH SCHOOL DIPLOMA OR GED CERTIFICATE AT 24-MONTH INTERVIEW, FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Adjusted Percentages, by Group		
		Experimental	Comparison	Difference
Black Teens	(N=306)	21	19	2
Mexican American Teens	(N=163)	19	16	3
Puerto Rican Teens	(N=115)	13	10	3
Teens Age 15 or Younger at Baseline	(N=221)	8	6	2
Teens Age 16-17 at Baseline	(N=440)	26	26	0
Teens Pregnant at Baseline	(N=407)	18	20	-2
Teens not Pregnant at Baseline	(N=254)	23	21	2
Teens in an AFDC Household at Baseline	(N=441)	19	17	2
Teens not in an AFDC Household at Baseline	(N=231)	22	24	-2
Teens in School at Baseline	(N=371)	21	24	-3
Teens not in School at Baseline	(N=290)	20	11	9*
Boston/Hartford Teens	(N=110)	16	8	8
Harlem/Bedford Stuyvesant Teens	(N=179)	22	19	3
Phoenix/San Antonio Teens	(N=288)	20	20	0
Riverside/Fresno Teens	(N= 84)	29	32	-3
Sample I Teens	(N=372)	23	23	0
Sample II Teens	(N=289)	16	16	0
All Teens	(N=661)	20	20	0

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The percentages shown have been adjusted for ethnicity, school status at baseline, highest grade completed at baseline, number of baseline jobs, number of times dropped out of school at baseline, enrollment in a teen-parent school program at baseline, and mother's education.

*Statistically significant at the .05 level.

TABLE 5.7

ADJUSTED PERCENTAGES OF TEENS EVER ENROLLED IN SCHOOL BETWEEN BASELINE AND 24-MONTH INTERVIEW, FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Adjusted Percentages, by Group		
		Experimental	Comparison	Difference
Black Teens	(N=313)	93	82	11**
Mexican American Teens	(N=163)	84	63	21***
Puerto Rican Teens	(N=119)	79	41	38***
Teens Age 15 or Younger at Baseline	(N=224)	85	73	12**
Teens Age 16-17 at Baseline	(N=450)	87	69	18***
Teens Pregnant at Baseline	(N=416)	86	70	16***
Teens not Pregnant at Baseline	(N=258)	87	71	16***
Teens in an AFDC Household at Baseline	(N=441)	86	70	16***
Teens not in an AFDC Household at Baseline	(N=231)	90	69	21***
Teens in School at Baseline	(N=375)	95	93	2
Teens not in School at Baseline	(N=299)	72	38	34***
Boston/Hartford Teens	(N=114)	76	43	33***
Harlem/Bedford Stuyvesant Teens	(N=186)	92	76	16**
Phoenix/San Antonio Teens	(N=289)	89	73	16***
Riverside/Fresno Teens	(N= 95)	88	83	5
Sample I Teens	(N=385)	84	73	11**
Sample II Teens	(N=289)	92	67	25***
All Teens	(N=674)	87	71	16***

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The percentages have been adjusted for ethnicity, school status at baseline, highest grade completed at baseline, age at first birth, enrollment in a teen parent program at baseline, number of baseline pregnancies, and number of jobs held at baseline.

**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

at baseline: nearly twice as many experimental dropouts (72%) as comparison dropouts (38%) returned to school after baseline.

The next educational outcome examined was the number of semesters a teen was enrolled in an educational program subsequent to the baseline interview. In this analysis, only complete semesters were measured; a teen was considered not to have been enrolled in any semester if she dropped out before completing it. As shown in the summary Table 5.4, experimental teens were enrolled for significantly more semesters than comparison teens at both the 12- and 24-month interviews. By the end of the study, experimental teens had been enrolled for a little more than two full semesters, while comparison teens had been enrolled about one and a half. Since only full semesters were counted, this finding really means that about half the experimental group spent a full extra semester in school relative to the comparison group. Once again, as shown in Table 5.8, this gain was experienced by experimental teens in virtually every subgroup. For teens not in school at baseline, experimental teens spent more than twice as many full semesters in school as comparison teens (1.45 versus 0.61). The differences were also sizable for Puerto Rican teens, who were predominantly in the Boston/Hartford sites.

Finally, the effect of the program on educational aspirations was examined. At the 12-month interview, about two out of five teens said they wanted more than a high school diploma or GED certificate. By the final interview almost half the teens reported such aspirations. The experimental and comparison group differences were not statistically significant.

All of the outcome measures shown in Table 5.4 use the date of the baseline interview as a reference point. In order to compare the results

TABLE 5.8

ADJUSTED MEAN NUMBER OF SEMESTERS ENROLLED IN AN EDUCATIONAL PROGRAM BETWEEN BASELINE AND 24 MONTH INTERVIEW FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Adjusted Means, by Group		
		Experimental	Comparison	Difference
Black Teens	(N=306)	2.49	2.01	0.48**
Mexican American Teens	(N=164)	1.84	1.27	0.57*
Puerto Rican Teens	(N=115)	1.38	.61	0.77*
Teens Age 15 or Younger at Baseline	(N=221)	2.20	1.85	0.35*
Teens Age 16-17 at Baseline	(N=440)	1.93	1.49	0.44**
Teens in an AFDC Household at Baseline	(N=428)	2.01	1.53	0.48***
Teens not in an AFDC Household at Baseline	(N=230)	2.22	1.62	0.60**
Teens Pregnant at Baseline	(N=407)	2.12	1.61	0.51**
Teens not Pregnant at Baseline	(N=254)	1.96	1.54	0.42*
Teens in School at Baseline	(N=371)	2.53	2.27	0.26+
Teens not in School at Baseline	(N=290)	1.45	0.61	0.84***
Boston/Hartford Teens	(N=110)	1.29	0.60	0.69*
Harlem/Bedford Stuyvesant Teens	(N=179)	2.29	1.68	0.61**
Phoenix/San Antonio Teens	(N=288)	2.23	1.81	0.42*
Riverside/Fresno Teens	(N= 84)	2.11	1.81	0.30
Sample I Teens	(N=372)	2.22	1.83	0.39*
Sample II Teens	(N=289)	1.87	1.25	0.62**
All Teens	(N=661)	2.05	1.58	0.47***

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The means shown have been adjusted for ethnicity, school status at baseline, highest grade completed at baseline, number of baseline jobs, number of times dropped out of school at baseline, enrollment in a teen-parent school program at baseline, and marital status at baseline.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

of this research with those from other studies, school status was also measured at fixed intervals after termination of the index pregnancy. As in the previous chapter, we present the results both for all cases for whom data were available at the specified interval (Panel A of Table 5.9) and for the subgroup of teens for whom information was available at all three time periods (Panel B of Table 5.9). In both sets of analyses the results are similar, and similar to results obtained when the outcomes were measured relative to baseline: program impacts were initially strong and diminished over time. The fact that the results in the two panels are nearly identical is consistent with the findings shown in Tables 5.5 to 5.8, which indicate that program effects were comparable for teens who were pregnant and teens who were mothers at baseline.

As indicated in Chapter 2, analyses were also conducted in which program participation was measured in ways other than a simple experimental/comparison dichotomy. Specifically, as shown in the regression tables in Appendix E (Tables E.15 to E.21) the effects of length of program enrollment and receipt of educational counseling services from Project Redirection on educational outcomes was assessed. These analyses generally indicated more positive program impacts than those shown in Table 5.4, but the meaning of these analyses is difficult to interpret because of the possibility of selection biases (see Chapter 2.D for a discussion of this issue). Nevertheless, since these analyses have potential programmatic and policy relevance, it is useful to discuss them briefly.

For the majority of educational outcomes, significant program impacts observed in Table 5.4 were stronger when the alternative treatment

TABLE 5.9

ADJUSTED RATES OF POSITIVE SCHOOL STATUS AT 12, 18 AND 24 MONTHS POSTPARTUM[a], BY EXPERIMENTAL AND COMPARISON GROUP[b]

Educational Outcome	Adjusted Mean or Percentage, by Group		
	Experimental	Comparison	Difference
A. Entire Sample[c]			
Percent in School/Completed 12 Months Postpartum (N=673)	51	41	10**
Percent in School/Completed 18 Months Postpartum (N=589)	46	38	8*
Percent in School/Completed 24 Months Postpartum (N=345)	45	43	2
B. Sample for Whom 24-Month Post-Partum Data Were Available[c]			
Percent in School/Completed 12 Months Postpartum (N=345)	52	42	10*
Percent in School/Completed 18 Months Postpartum (N=345)	45	36	9+
Percent in School/Completed 24 Months Postpartum (N=345)	45	43	2

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Postpartum refers to the period following the index pregnancy. The index pregnancy was either the pregnancy in progress at the time of the baseline interview or the most recently terminated pregnancy at baseline.

[b] The percentages in this table have been adjusted for ethnicity, school status at baseline, baseline participation in a teen parent program, highest grade completed at baseline, number of times dropped out of school at baseline, and amount of time between baseline and termination of the index pregnancy. The full regression tables are presented in Tables E.19 to E.21 of Appendix E.

[c] Panel A presents data for every teen for whom school status information was available at the specified interval. Since many teens were pregnant at baseline, 24 months following the pregnancy had not yet elapsed at the time of the final interview for these teens. Panel B presents data for a constant cohort for whom data were available at all three intervals.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.

measures were used, and nonsignificant impacts become significant. For example, the average increment in the total number of semesters enrolled associated with program participation was .47, as shown in Table 5.4. For teens who obtained educational counseling from the program, the average increase was .83 semesters. When number of months enrolled in the program was substituted for the dichotomous group variable it was learned that each month of participation was associated with a .05 semester gain, other factors being equal (see analyses (2) and (3) of Table E.18).

For other outcomes, the alternative participation variables reflected significant impacts where the dichotomous variable had not. For example, for the analysis predicting positive school status (i.e. in school or completed) at the final interview, the group (experimental/comparison) variable was not significant. However, receipt of educational counseling from Project Redirection had a substantial impact, significant at the .01 level (see Table E.15). Receipt of educational counseling from other sources also improved the teens' educational status at follow-up. Similar results were obtained for school status 24 months postpartum.

Teens who were enrolled in Project Redirection for at least 12 months had especially favorable educational outcomes relative to comparison teens. As indicated in Chapter 2, long-term enrollees were actually more similar to comparison teens at baseline than were teens in the aggregated experimental sample, and therefore the possibility of substantial selection biases does not seem likely, especially if statistical adjustments are made for any existing initial differences. Below are some adjusted comparison of long-term enrollees with comparison teens (all of these differences were statistically significant at or beyond the .05 level):

	Experimentals Enrolled ≥ 12 Months	Comparison Teens	Difference
• Percent in School or Completed at 24-Month Interview	53%	43%	10%
• Percent Completed School at 24-Month Interview	27%	20%	7%
• Percent Ever Enrolled in School Post-Baseline	94%	71%	23%
• Mean Number of Semesters in School Post-Baseline	2.48	1.58	0.90

One further set of analyses explored program impacts for teens who either did or did not have a pregnancy subsequent to the index pregnancy. Since programs did not affect rates of repeat pregnancy, and since a new pregnancy was assumed to affect school enrollment, such an analysis seems justifiable. Results are presented in Table 5.10. Several aspects of this table deserve comment. First, a higher-order pregnancy was clearly associated with more negative outcomes. If the comparison group is viewed as a basis for what would "normally" be expected, it can be seen that those who avoided a subsequent pregnancy fared much better than those who did not (for example, 24% versus 12%--exactly twice as many--had finished schooling). Second, among the teens with no subsequent pregnancy, program impacts were marginal. For those who experienced another pregnancy during the study period, there were several significant program impacts. In this subgroup, for example, 36% more experimental than comparison teens had been in school (86% versus 63%) and 67% more had completed school (20% versus 12%). Finally, for these same two outcomes, participation reduced or eliminated the deficits associated with another pregnancy. For example, among Redirection participants a comparable percent of teens with and without another pregnancy completed school and had enrolled in school post-baseline. Thus, participation in the program appears to have

TABLE 5.10

SELECTED EDUCATIONAL OUTCOMES, FOR TEENS WITH OR WITHOUT A SUBSEQUENT PREGNANCY[a,b]

Selected Outcome	Adjusted Mean or Percentage, by Group			
	Teens with a Subsequent Pregnancy		Teens Without a Subsequent Pregnancy	
	Experimental	Comparison	Experimental	Comparison
Percent in School or Completed School at 24-Month Interview	41+	31	46	53
Percent Received GED/ Diploma at 24-Month Interview	20+	12	22	24
Percent Ever in School Post-Baseline	86***	63	85*	76
Mean Number of Semesters Enrolled Post-Baseline	1.94***	1.30	2.16*	1.81
Number of Respondents	149	163	154	201

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] The figures in this table have been adjusted for relevant baseline characteristics; different covariates were used for different outcomes.

[b] "Subsequent pregnancy" refers to any pregnancy after the index pregnancy. The index pregnancy was either the pregnancy in progress at the time of the baseline interview or the most recently terminated pregnancy at baseline.

*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

buffered the impact of another pregnancy on these teens' school-related behaviors.

In summary, for the sample as a whole, experimental teens were not significantly different from comparison group teens with respect to several important educational outcomes at the end of the study. When background characteristics were adjusted, similar percentages of teens in the two groups were in an educational program or had completed their basic schooling at the time of the 24-month interview.

Nevertheless, the data strongly suggest that Project Redirection had a powerful influence on teens' educational behaviors while in the program: significantly more experimental than comparison teens had stayed in or returned to school post-baseline; they also had spent more time enrolled in an educational program. At 12 months after baseline (and at 12 and 18 months postpartum), significantly more experimental teens than comparison teens were either in school or had completed it. At 24 months, however, when teens were no longer in the program, the group differences were not sustained. The extra schooling that the teens obtained during the two years under study did not result in higher rates of school completion.

While the program impacts at the end of the study were generally disappointing, there were two subgroups for whom experimental/comparison differences were substantial: school dropouts and teens with a subsequent pregnancy. The program appeared to have a consistent and enduring beneficial impact on teens who were not in school at the time they enrolled in the program. For every outcome studied, these teens demonstrated substantially improved educational behaviors relative to their comparison counterparts. Of course, relative to teens who were initially in school in either group, the dropouts remained at a disadvantage. However, participation in Redirection reduced or, in the case of school completion, eliminated this disadvantage. The program had a similar buffering effect on teens who had another pregnancy during the two-year study period.

Further analyses revealed that long-term educational outcomes were more positive for teens who stayed in the program longer and who received educational counseling. While these findings must be treated cautiously because more highly motivated teens may have more fully availed themselves of these program opportunities, the data are nevertheless consistent with an explanation that ongoing, targeted program services were especially likely to improve the teens' educational experiences.

CHAPTER 6

PROGRAM IMPACTS ON EMPLOYMENT OUTCOMES

Young women who become pregnant and keep their children are often dependent on public welfare for economic support. If these young women drop out of school, as many do, and if they are members of a minority group, they are especially at-risk to long-term poverty and welfare dependence (Bane and Ellwood, 1983; Furstenberg, 1976; Haggstrom et al., 1981; Moore, 1978; Presser, 1980). In fact, it has been calculated that more than half of all AFDC expenditures are to households in which the mother was a teenager when her first child was born (Moore, 1978; Block, 1981; Scheirer, 1981; New York State Temporary Commission to Revise Social Services Law, 1983). Several researchers have estimated that annual public costs for such households are close to $10 billion (Moore and Burt, 1982; SRI International, 1979). Another investigator estimated that in 1975 a total of about $150 million would have been saved if the AFDC recipients in her sample had postponed childbearing by a single year (Scheirer, 1981).

There are two primary routes to self-sufficiency for welfare recipients—through employment earnings or marriage. For the target population of Project Redirection—poor, mostly minority teenage mothers—the more probable path is through employment (Bane and Ellwood, 1983; Chambre, 1977). Yet the obstacles these teens face in securing employment are enormous. They bring to the labor force little in the way of human capital: they lack educational credentials, have little prior work experience, and have few marketable skills. They may also lack adequate child care arrangements. As members of minority groups, many face the additional problem of racial discrimination. Black females, for example, had the highest rate of unemployment and involuntary part-time work of any combined sex and ethnicity group in 1980. Furthermore, black women were more often in marginal jobs and in poverty households even when they worked. Hispanic females had similar employment problems (U.S. Commission on Civil Rights, 1982).

Early work experience and job training are critical to the future employability of young mothers. There is evidence that the more training a job requires, the lower the level of unemployment (U.S. Commission on Civil Rights, 1982). Furthermore, analyses of longitudinal data have shown that adult employability and earnings are positively related to opportunities to gain job experience during youth. For example, Stephenson (1979), using data from the National Longitudinal Surveys of Labor Market Experience (NLS), found that work during secondary school was associated with lower unemployment in later life. Similar findings have been reported by other investigators (e.g. Coleman, 1976; Farkas _et al_., 1984; Johnston and Bachman, 1973; Stevenson, 1978). McLaughlin (1977), in his path analysis of NLS data, found that early work experience had a particularly strong effect on the earnings potential of women who became mothers before age 19.

In keeping with its long-term goal of self sufficiency, Project Redirection included an employability component. This feature is an unusual one for a teen-parent program, even among comprehensive programs. Only a handful of programs have focused on employment-related services (e.g. Schinke et al., 1978, 1980; Goldstein et al., 1973). Project Redirection's employability component was included in the original design of the demonstration, but was considerably strengthened during its second phase of operations. During Phase II, program guidelines called for a minimum of 18 hours of employment-related activities for each client. (For more detail see Branch et al., 1984.)

The issue of employment training for school-age parents is one that has stirred some controversy. Some commentators argue that the top priorities for these young women are to be good mothers and to finish high school, and that employment might interfere with these goals. Employment per se was not, in fact, one of Project Redirection's objectives. The program's emphasis was on employability development--i.e. the acquisition of employability skills and motivation to work. Nevertheless, several actual employment outcomes were included in this impact analysis. The primary rationale for their inclusion is that while the program did not promote immediate employment, the development of job skills through parttime or temporary employment was considered a positive step toward eventual regular employment and self-sufficiency.

This chapter, then, examines the employment-related experiences of the teens in the impact analysis sample. The first section discusses the teens' work behaviors and attitudes as they evolved over the two-year study period, and also examines ethnic group differences in employment. The next section focuses on the impact of Project Redirection on the teens' work behaviors. The concluding section examines experimental and comparison group differences with regard to job-related "enabling factors"--i.e., job readiness factors that are presumed to facilitate employment. Appendix D presents some information on the employment experiences of teen parents in other studies.

A. Employment-Related Behaviors and Attitudes: An Overview

At each interview teens were asked if they were currently employed. As shown in Table 6.1, only a minority of teens were actually employed at any interview, although the percent employed increased by about 60% (from 9.6% to 15%) between baseline and the final interview. It should be recalled, however, that these interviews were scheduled almost exclusively during the academic year, when about half of these teens were still in school.

Most teens, however, had had some work experience. Even at baseline, some 62% of the sample had worked for pay at some point in their lives. By the final interview, nearly 80% had been employed. In all three interviews, employment was predominantly in non-skilled and low-paying jobs. The most commonly cited types of employment for the most recent job held at the final interview were fast food clerk (14%), file or general clerk (12%), cashier (8%), janitorial/cleaning work (10%), day care assistant/ babysitting (9%), stock or sales clerk (5%), camp counseling (4%), and factory work (5%). On average, the teens in this sample had held two different jobs and had accumulated about 35 weeks of work experience by the time of the final interview. Their average hourly wage was at about the minimum wage, $3.38.

As shown in Table 6.1, nearly two out of five teens reported that they were looking for work at both follow-up interviews. When those who were seeking employment are combined with those actually employed, about half the sample can be described as being in the labor force at both the 12- and 24-month interviews. At the final interview, 48% of those seeking work wanted a full time job and an additional 17% said they wanted either full- or part-time employment.

The most commonly-used job search strategy was reading the want ads, reported by 58% of those looking for work. According to the reports of these unemployed teens, their job seeking was an active endeavor: three-fourths said they had completed a job application in the previous month and half had had a job interview. Among the 329 teens who were not in the labor force at 24 months post-baseline, the most commonly cited reasons for not seeking employment were child care (43%), school attendance (28%), and a current pregnancy (13%). Thirty percent of these teens, however, said they definitely intended to look for work in the next 12 months, and 17% said they probably would; some 37% said they definitely would not.

As shown in Table 6.1, about 70% of the teens at both the 12- and 24-month interviews felt that they would be working most of the time in the future. An additional 22% thought they would be working now and then, and 8% believed they would never have to work. Among those teens who felt that they would be working in the future, the most commonly cited occupational expectations were secretarial (13%), clerical (9%), cashiering (7%), nursing (8%), cosmetology (4%), typing (3%), operating a computer (4%), and sales work (4%). Thus, these teens generally envisioned themselves in jobs that are traditional for women but that often require specific skills or training, and at least a high school or GED degree.

The teens in this study were also administered several scales measuring employment attitudes and knowledge. The Employability Knowledge

Test is a 17-item test prepared by the Educational Testing Service (ETS) that measures comprehension in such areas as completing a job application and reading want ads. At baseline, Sample I teens averaged 10.6 correct answers out of a possible 17. Between baseline and the 24-month interview, scores on this test increased by about 1.5 points, a 15% improvement. Most of this improvement was realized in the last 12 months of the study. The mean score at the final interview was about a half-point higher than the comparably-aged standardization sample for this instrument.

Table 6.1 also shows scores on another scale included in an ETS battery of employment-related tests for teenage students, the Career Maturity Test. This scale consists of 30 items that measure decisiveness and personal planning relating to career choice.[1] At baseline, the mean score was 18.8, a score comparable to the scores observed for other samples of minority youth. Twelve months later, the mean score on this test declined somewhat, but by the final interview there was an average increase of about 1 1/2 points over baseline scores.

A five-item Likert scale was also included to measure attitudes toward work that is nontraditional for women.[2] On this scale scores could range from a low of 5 (negative attitudes) to a high of 20 (positive

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[1] Two typical items from this inventory include the following: "You shouldn't worry about choosing a job since you don't have anything to say about it anyway" and "Entering one job is about the same as entering another." For both items, disagreement is scored as more "career mature" than agreement.

[2] An example of an item on this scale is as follows: "No real woman would want to do men's work, like construction or auto repair." Strong disagreement was scored 4; strong agreement was scored 1.

TABLE 6.1

SELECTED EMPLOYMENT-RELATED VARIABLES FOR THE IMPACT ANALYSIS SAMPLE AT BASELINE, 12-MONTH, AND 24-MONTH INTERVIEWS[a]

Selected Variable	Baseline Interview		12-Month Interview		24-Month Interview	
	Sample I Teens	All Teens	Sample I Teens	All Teens	Sample I Teens	All Teens
Percent Currently Employed		9.6		12.7		15.0
Percent Ever Employed		62.1		74.1		79.4
Mean Number of Jobs Held		1.0		1.4		2.1
Mean Number of Weeks Ever Worked		17.7		25.9		34.8
Mean Hourly Wage, Most Recent or Current Job		--		$3.26		$3.38
Percent Currently Looking for Work[b]		--		41.6		36.3
Percent Expecting to be Employed Most of the Time in the Future[b]		--		71.0		70.7
Mean Score, Employability Knowledge Test	10.6	--	10.7	10.8	12.2	11.9
Mean Score, Career Maturity Test	10.8	--	18.3	18.4	20.2	20.1
Mean Score, Attitudes Toward Nontraditional Employment	13.7	--	13.3	13.5	14.4	14.4

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] When baseline information could be obtained retrospectively for Sample II respondents, the information shown is for the entire impact aalysis sample (N=675). When baseline information was available only for Sample I respondents, baseline figures are shown only for Sample I (N=383). and follow-up information is shown both for Sample I and the aggregated impact analysis sample separately.

[b] Information for this variable was not obtained in the baseline interview.

attitudes), the theoretical midpoint being 12.5. At baseline, the mean score on this attitude scale was 13.7, indicating fairly neutral attitudes toward women entering nontraditional fields. By the final interview, scores reflected somewhat more favorable attitudes toward nontraditional work, as shown in Table 6.1.

Ethnic group differences in employment-related behaviors and attitudes tended to be substantial, as shown in Table 6.2. More than one-fifth of the white and Mexican American teens were employed at the final interview, primarily in full-time jobs. By contrast fewer than one tenth of the Puerto Rican teens were working.

The majority of teens in all four major ethnic groups had worked for pay at some point in their lives, but job experience was greatest for whites and Mexican Americans and lowest for Puerto Ricans. White teens had held over three jobs, on average, and had worked an average of 59 weeks in their lifetimes. Puerto Rican teens had half the number of jobs as white teens (1.5) and had accumulated fewer than half as many weeks (24) of work experience. In all ethnic groups, the hourly wage of the most recent job worked was at about the minimum wage.

Black teens were most likely to say they were currently looking for work, and Puerto Ricans were least likely to say so. When those employed are combined with those seeking work, rates of labor force participation are found to be comparable for black (60%) and white (56%) teens, lower for Mexican Americans (49%), and substantially lower for Puerto Ricans (28%). Among those teens in the labor force, rates of unemployment were highest by far for black teens (80%). Among those teens not currently employed, black teens were most likely to have applied for a job in the past month.

TABLE 6.2

SELECTED EMPLOYMENT OUTCOMES AT 24-MONTH INTERVIEW, BY ETHNICITY

Employment Status Variable	Black Teens	Mexican American Teens	Puerto Rican Teens	White Teens	All Teens[a]
Percent Currently Employed More Than 20 Hours Per Week	8.9	17.1	5.9	16.9	11.4***
Percent Currently Employed 20 Hours or Fewer Per Week	2.9	4.9	1.7	6.8	3.6**
Percent Ever Employed	78.9	82.9	70.6	86.4	79.4*
Mean Number of Jobs Ever Held	2.1	2.1	1.5	3.1	2.1***
Mean Number of Weeks Ever Worked	32.9	37.6	23.9	59.0	34.8***
Mean Hourly Wage, Most Recent or Current Job	$3.40	$3.36	$3.25	$3.37	$3.38
Percent Currently Looking for Work[b]	54.7	35.2	21.8	42.2	42.5***
Percent Applied for Job in Past Month[b]	39.0	28.9	13.3	26.7	30.6***
Percent not Employed or Looking for Work Because of Child Care[c]	27.4	44.6	64.7	34.6	42.9***
Percent not Employed or Looking for Work Becuase Work not Wanted[c]	5.6	16.9	8.2	11.5	9.4
Percent Expecting to be Employed Most of the Time in the Future	81.9	64.6	44.1	77.2	70.7***
Mean Score, Employability Knowledge Test	12.4	12.0	9.8	13.6	11.9***
Mean Score, Career Maturity Test	20.0	20.2	17.9	23.7	20.1***
Mean Score, Attitudes Toward Nontraditional Employment	14.5	14.4	13.5	15.7	14.4***
Number of Respondents	313	164	119	59	675

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at 24 months after baseline.

NOTES: [a]The last column presents information for all teens in the sample, including 20 teens whose ethnicity was not black, Mexican American, Puerto Rican, or white.

[b]These percentages are based on responses from unemployed teens (N=576).

[c]These percentages are based on responses from unemployed teens who were not looking for work (N=329).

*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

Teens not in the labor force were asked their reasons for not working or seeking work at the time of final survey. As shown in Table 6.2, Puerto Rican teens were substantially more likely than any other teens to cite child care as the major reason for not being in the labor force, although all groups cited this as a key factor influencing their decision. Only a small minority in all four groups said they just did not want to work, and ethnic group differences for this response were not statistically significant. When asked about future work plans, a substantial majority of black teens and white teens said they expected to be working most of their lives, and more than half of the Mexican Americans also foresaw employment in their futures. Fewer than half the Puerto Rican teens thought they would be working most of their adult lives.

With regard to employment attitudes and knowledge, ethnic group differences tended to be similar in direction to differences in employment behavior. White teens had the highest scores on the Employability Knowledge Test, the Career Maturity Test and the Attitudes Toward Nontraditional Employment scale. Puerto Rican teens had the lowest scores on all three measures.

When the employment status of the teens in this sample is considered relative to the termination of the index pregnancy, the picture remains fairly similar. As shown in Table 6.3, Puerto Rican teens were least likely of any ethnic group to be employed at 12, 18 and 24 months postpartum. However, this table highlights some additional patterns. Among the Mexican American and black teens, the percentage employed increased between 12 and 24 months postpartum, while among the white and Puerto Rican teens, the percentage declined.

TABLES 6.3

EMPLOYMENT STATUS AT SELECTED INTERVALS SUBSEQUENT TO INDEX PREGNANCY, BY ETHNICITY

Employment Status Variable	Black Teens	Mexican American Teens	Puerto Rican Teens	White Teens	All Teens[a]
Percent Employed 12 Months Postpartum[b]	10.3 (311)[c]	18.3 (164)	12.7 (118)	24.1 (58)	14.0** (671)
Percent Employed 18 Months Postpartum	18.3 (279)	19.7 (147)	7.1 (98)	30.2 (53)	18.0** (596)
Percent Employed 24 Months Postpartum	17.6 (176)	32.5 (77)	4.9 (61)	18.2 (33)	18.4** (359)

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] The last column presents information for all teens in the sample, including teens whose ethnicity was not black, Mexican American, Puerto Rican, or white.

[b] Postpartum refers to the period following the index pregnancy. The index pregnancy was either the pregnancy in progress at the time of the baseline interview or the most recently terminated pregnancy at baseline.

[c] The number in parentheses indicates the number of teens about whom information was available. Since some teens were pregnant at baseline, the employment status of all teens 18 and 24 months postpartum could not be ascertained during the 24-month post-baseline interview.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

Because the school, work, and fertility behaviors of these teens were expected to be intricately intertwined, a further analysis examined the school and employment status of teens who, by the final interview, either did or did not have a pregnancy subsequent to the index pregnancy. The results are shown in Table 6.4. Overall. only about 5% of the sample had completed their basic schooling and were working at the final interview. More than two and a half times as many of the teens who avoided a subsequent pregnancy as those who did not had completed school and were working. A similar margin of difference between those with and without a subsequent pregnancy was found for employed teens who were either still in school or who had dropped out prior to completion.

Among the unemployed teens who said they were seeking employment, the differences between those with and without a repeat pregnancy were again substantial. For teens with a positive school status (in school or completed), those with another pregnancy were about half as likely to be looking for work as those without one (10.3% versus 21.1%).

Teens in both pregnancy groups were most likely to be neither working, nor in school. However, the rate of teens in the final category in Table 6.4 was nearly twice as high for the repeat pregnancy group. Nearly half of those with a pregnancy subsequent to baseline were not in school, had not completed school, were not working, and were not looking for work. It is, of course, impossible to conclude from these data whether teens left school and were not seeking work because of a repeat pregnancy, whether teens who were out of school and work had more opportunity for a subsequent pregnancy, or whether personal characteristics or circumstances influenced work, school, and fertility behaviors jointly. It is clear

TABLE 6.4

SCHOOL AND WORK STATUS OF TEENS WITH AND WITHOUT A REPEAT PREGNANCY AT THE 24-MONTH INTERVIEW

School and Work Status	Teens Without A Repeat Pregnancy	Teens With A Repeat Pregnancy	Difference
Percent Completed School and Employed	8.6	3.2	5.4
Percent in School and Employed	4.7	1.9	2.8
Percent not in School or Completed but Employed	7.2	3.5	3.7
Percent Completed and Looking for Work	8.6	4.5	4.1
Percent in School and Looking for Work	12.5	5.8	6.7
Percent not in School or Completed but Looking for Work	20.5	18.9	1.6
Percent Completed School but not in the Labor Force	5.3	8.0	- 2.7
Percent in School and not in the Labor Force	10.2	12.2	- 2.0
Percent Neither in School nor in the Labor Force	22.4	42.0	-19.6
Total	100.0	100.0	
No. of Respondents	361	312	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II 24 months after baseline.

NOTES: The percentages may not add to 100.0 percent due to rounding error.

The overall difference between those with and without a pregnancy subsequent to the index pregnancy is statistically significant at the .0001 level.

from Table 6.4, however, that a repeat pregnancy was associated with especially poor educational and work-related outcomes.

In summary, relatively few teens in this sample were employed at the final interview, although many had been able to find work during the two-year study period. Most often their work experience had been in low-paying, unskilled jobs that were held only two or three months, usually during the summer. Many teens reported at the 24-month interview that they were actively seeking work. Ethnic group differences relating to employment were substantial. Puerto Rican teens had particularly low rates of work experience and job seeking. The school and labor force participation rates of the teens in this sample were strongly affected by whether or not they had had a repeat pregnancy. Some 42% of those with another pregnancy, compared with 22% of those without another pregnancy, were neither in school nor in the labor force at the final interview. The next section examines the impact of Project Redirection on employment-related behaviors.

B. Redirection Impacts on Employment-Related Behaviors

Adjusted employment-related outcomes for experimental and comparison teens are presented in Table 6.5.[3] The first two outcomes in this table are variables that take the teens' school and work status at the final interview into account simultaneously. The first is the percent of teens

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[3] Other employment outcomes analyzed but not shown in Table 6.5 include the following: mean number of weeks ever worked at 12- and 24-month interview; mean wage of current or most recent job at both follow-up interviews; and labor-force participation at both interviews. Group differences were non-significant in all instances after controlling background variables.

TABLE 6.5

SUMMARY OF ADJUSTED EMPLOYMENT OUTCOMES, BY EXPERIMENTAL AND COMPARISON GROUPS[a]

Employment Variable	Adjusted Mean or Percentage, by Group		
	Experimental	Comparison	Difference
Percent Either in School/Completed or Employed, 24-Months Post-Baseline	51	48	3
Percent Either in School/Completed or in the Labor Force[b], 24-Month Post-Baseline	74	65	9**
Percent Employed 12 Months Post-Baseline	14	12	2
Percent Employed 24 Months Post-Baseline	15	15	0
Mean Number of Jobs Held, 12 Months Post-Baseline	1.47	1.25	0.22**
Mean Number of Jobs Held, 24 Months Post-Baseline	2.16	1.90	0.26*
Percent Ever Employed, Baseline to 12 Months	49	38	11**
Percent Ever Employed, Baseline to 24 Months	61	54	7+
Percent in an AFDC Household 12 Months Post-Baseline	70	70	0
Percent in an AFDC Household 24 Months Post-Baseline	75	68	7+
Percent Receiving own AFDC Grant 24 Months Post-Baseline	61	57	4

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]The means and percentages in this table have all been adjusted for ethnicity, age, school status at baseline, and baseline work experience. Various other characteristics were also controlled, but different covariates were required for different outcomes. The full regression tables are presented in Tables E.22 to E.27 of Appendix E for all 24-month outcomes. All covariates significantly related to the outcomes, as shown in these appendix tables, were controlled in deriving the figures presented in this table.

[b]A teen was considered to be in the labor force if she was currently employed or reported that she was seeking employment.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.

who were either working or in school (or had completed their basic schooling). In both the experimental and comparison groups, nearly half the teens could be thus classified. The 3% group difference was not statistically significant.

The next variable includes those teens who were looking for work. Three-fourths of the experimentals, but only 65% of the comparison group teens had either a positive school or work status at the 24-month interview. In other words, 14% more of the experimental teens were engaged in some behavior at the final interview that could be construed as directing their lives toward self-sufficiency (i.e. through schooling, employment or job seeking). The difference of nine percentage points was significant at the .01 level.

Subgroup results for these two joint school/work variables are presented in Tables 6.6 and 6.7. The first of these tables shows that for several subgroups, the experimental teens were more likely than the comparison teens to either have a positive school status or be working at the final interview. In every case, these subgroups represent teens who entered the study with the greatest disadvantages in terms of school history, employment history, and home environment. That is, high school dropouts, teens with no work experience, teens who had had a very early pregnancy, and Puerto Rican teens appear to have been positively affected by participation in Project Redirection with respect to combined school/work behaviors, even after they ceased to be served by the program.

When job hunting is added to the picture, program impacts were observed for many of the subgroups examined, as shown in Table 6.7. In every subgroup, experimental teens had higher percentages with a positive school/work status at the final interview than comparison teens. The

TABLE 6.6

ADJUSTED PERCENTAGES OF TEENS EITHER IN SCHOOL/COMPLETED OR WORKING AT THE 24-MONTH INTERVIEW, FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Adjusted Percentages, by Group		
		Experimental	Comparison	Difference
Black Teens	(N=306)	60	58	2
Mexican American Teens	(N=161)	38	42	- 4
Puerto Rican Teens	(N=118)	34	19	15+
Teens Age 15 or Younger at Baseline	(N=219)	55	45	10+
Teens Age 16-17 at Baseline	(N=442)	46	49	- 3
Teens Pregnant at Baseline	(N=411)	51	48	3
Teens not Pregnant at Baseline	(N=250)	49	47	2
Teens in an AFDC Household at Baseline	(N=441)	48	44	4
Teens not in an AFDC Household at Baseline	(N=231)	54	54	0
Teens in School at Baseline	(N=371)	61	64	- 3
Teens not in School at Baseline	(N=290)	36	25	11*
Teens With Work Experience at Baseline	(N=411)	53	55	- 2
Teens Without Work Experience at Baseline	(N=250)	45	35	10+
Boston/Hartford Teens	(N=113)	37	19	18*
Harlem/Bedford Stuyvesant Teens	(N=181)	58	53	5
Phoenix/San Antonio Teens	(N=181)	50	54	- 4
Riverside/Fresno Teens	(N= 84)	55	53	2
Sample I Teens	(N=377)	53	46	7
Sample II Teens	(N=284)	45	50	- 5
All Teens	(N=661)	51	48	3

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The percentages shown have been adjusted for ethnicity, school status at baseline, AFDC status at baseline, number of baseline jobs, and employment status at baseline, highest grade completed at baseline, and number of semesters in a teen parent program at baseline.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.

TABLE 6.7

ADJUSTED PERCENTAGES OF TEENS EITHER IN SCHOOL/COMPLETED OR IN THE LABOR FORCE AT THE 24-MONTH INTERVIEW[a], FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Adjusted Means, by Group		
		Experimental	Comparison	Difference
Black Teens	(N=306)	82	78	4
Mexican American Teens	(N=161)	69	59	10
Puerto Rican Teens	(N=118)	51	29	22**
Teens Age 15 or Younger at Baseline	(N=219)	73	65	8
Teens Age 16-17 at Baseline	(N=442)	73	64	9*
Teens Pregnant at Baseline	(N=411)	76	66	10**
Teens not Pregnant at Baseline	(N=250)	70	64	6
Teens in an AFDC Household at Baseline	(N=441)	70	64	6
Teens not in an AFDC Household at Baseline	(N=231)	78	66	12*
Teens in School at Baseline	(N=371)	81	79	2
Teens not in School at Baseline	(N=290)	62	46	16***
Teens With Work Experience at Baseline	(N=411)	77	71	6
Teens Without Work Experience at Baseline	(N=250)	67	54	13*
Boston/Hartford Teens	(N=113)	54	29	25**
Harlem/Bedford Stuyvesant Teens	(N=181)	83	67	16*
Phoenix/San Antonio Teens	(N=181)	76	74	2
Riverside/Fresno Teens	(N= 84)	77	74	3
Sample I Teens	(N=377)	76	65	11**
Sample II Teens	(N=284)	68	64	4
All Teens	(N=661)	74	65	9**

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The percentages shown have been adjusted for ethnicity, age at first birth, school status at baseline, AFDC status at baseline, number of baseline jobs, employment status at baseline, highest grade completed at baseline, and number of semesters in a teen parent program at baseline.

[a] A teen was considered to be in the labor force if she was currently employed or reported that she was seeking employment.

*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

differences reached levels of statistical significance for 10 of the 19 subgroups. As in previous analyses, there was a strong tendency for girls entering the study with the greatest disadvantages (e.g. being a school dropout) to be most affected by program participation.

Returning to the summary information presented in Table 6.5, it can be seen that actual employment rates at 12 and 24 months after baseline were low for both groups, and differences were small and nonsignificant. Table 6.8 presents adjusted percentages of teens employed at the 24-month interview for teens in various subgroups. For the 19 subgroups examined, differences between the experimental and comparison group were generally small and inconsistent in direction. However, in two subgroups a significant group difference favoring experimental teens did emerge. A finding of particular interest was that experimental teens who had been in an AFDC household at baseline were significantly more likely than similar comparison teens to be employed two years after entry into the study. Sixty percent more experimental (16%) than comparison teens (10%) who initially lived in a household with at least one AFDC recipient were employed at the end of the study. This analysis suggests yet again that program participation helped to reduce or eliminate early disadvantages: whereas employment among comparison teens was twice as high for girls not living in an AFDC household as for those who were (20% versus 10%), the two experimental subgroups had identical rates of 24-month employment (16%).

The second subgroup with significant experimental/comparison differences was the Phoenix/San Antonio teens. This outcome probably reflects the very strong employment component offered at the Phoenix site,

TABLE 6.8

ADJUSTED PERCENTAGES OF TEENS EMPLOYED AT THE 24-MONTH INTERVIEW, FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Adjusted Percentages, by Group		
		Experimental	Comparison	Difference
Black Teens	(N=306)	14	12	2
Mexican American Teens	(N=161)	20	23	- 3
Puerto Rican Teens	(N=118)	13	5	8
Teens Age 15 or Younger at Baseline	(N=219)	17	8	9
Teens Age 16-17 at Baseline	(N=442)	14	17	- 3
Teens Pregnant at Baseline	(N=411)	16	14	2
Teens not Pregnant at Baseline	(N=250)	17	15	- 1
Teens in an AFDC Household at Baseline	(N=441)	16	10	6*
Teens not in an AFDC Household at Baseline	(N=231)	16	20	- 4
Teens in School at Baseline	(N=371)	15	18	- 3
Teens not in School at Baseline	(N=290)	16	11	5
Teens With Work Experience at Baseline	(N=411)	17	17	0
Teens Without Work Experience at Baseline	(N=250)	11	11	0
Boston/Hartford Teens	(N=113)	12	4	8
Harlem/Bedford Stuyvesant Teens	(N=181)	12	9	3
Phoenix/San Antonio Teens	(N=181)	18	5	13+
Riverside/Fresno Teens	(N= 84)	18	11	7
Sample I Teens	(N=377)	15	14	1
Sample II Teens	(N=284)	14	18	- 4
All Teens	(N=661)	15	15	0

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The percentages shown have been adjusted for ethnicity, age of first birth, school status at baseline, AFDC status at baseline, number of baseline jobs, and the interval between termination of the index pregnancy and baseline.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.

which included an average of 20 weeks of full-time skills training at a community training center (see Branch et al., 1984, p. 56).

Although for the aggregated sample the program did not appear to affect teens' employment at the time of the 12- or 24-month interviews, participation in the program was found to be related to cumulative employment experience. Table 6.5 shows that at both follow-up interviews, experimental group teens had held significantly more jobs than comparison teens after adjusting for baseline work experience and other background characteristics. At the final interview, the mean adjusted difference was .26, suggesting that about a third of the experimentals had an additional job relative to comparison teens. In an adult sample, a result such as this one might be difficult to interpret, since holding more jobs could reflect greater employment instability. However, in a sample of teenagers whose employment occurs primarily during the summer months, this finding suggests that experimental teens were accumulating more (and perhaps more varied) work experience than comparison teens.

Table 6.5 also shows that participation in Project Redirection was associated with an increased incidence of employment subsequent to baseline. At the 12-month interview, nearly 30% more of the experimental (49%) than comparison (38%) teens had worked for pay since baseline. By the 24-month interview, many more teens in both groups had been employed. The group difference, though diminished to a 13% experimental advantage (61% versus 54%), was still significant.

Table 6.9 presents subgroup differences relating to post-baseline work experience. Experimental teens had higher rates of employment than comparison teens in virtually every subgroup, though the differences were statistically significant in only a few. Particularly noteworthy is the

TABLE 6.9

ADJUSTED PERCENTAGES OF TEENS WITH WORK EXPERIENCE SUBSEQUENT TO BASELINE, FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Adjusted Percentages, by Group		
		Experimental	Comparison	Difference
Black Teens	(N=306)	65	59	6
Mexican American Teens	(N=160)	59	59	0
Puerto Rican Teens	(N=118)	54	24	30**
Teens Age 15 or Younger at Baseline	(N=218)	60	50	10
Teens Age 16-17 at Baseline	(N=442)	61	57	4
Teens Pregnant at Baseline	(N=410)	58	50	8
Teens not Pregnant at Baseline	(N=250)	63	61	2
Teens in an AFDC Household at Baseline	(N=428)	63	48	15**
Teens not in an AFDC Household at Baseline	(N=230)	63	63	0
Teens in School at Baseline	(N=370)	68	65	3
Teens not in School at Baseline	(N=290)	49	40	9
Teens With Work Experience at Baseline	(N=411)	66	62	4
Teens Without Work Experience at Baseline	(N=249)	52	39	13+
Boston/Hartford Teens	(N=113)	53	26	27**
Harlem/Bedford Stuyvesant Teens	(N=181)	58	55	3
Phoenix/San Antonio Teens	(N=181)	65	45	20*
Riverside/Fresno Teens	(N= 84)	55	46	9
Sample I Teens	(N=376)	61	58	3
Sample II Teens	(N=284)	54	56	- 2
All Teens	(N=660)	61	54	7+

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The percentages shown have been adjusted for ethnicity, age of the youngest child, pregnancy status at baseline, number of baseline pregnancies, school status at baseline, AFDC status at baseline, number of semesters repeated at baseline and number of jobs held at baseline.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .001 level.

fact that 33% more experimental than comparison teens gained their first work experience during the two years since baseline (52% versus 39%). Additionally, among teens living in an AFDC household at baseline, a third more of the experimental (63%) than comparison teens (48%) held a paying job during the two-year study period. Here again, work experience was identical for the two welfare-status subgroups among experimental teens (63% for both), while for comparison teens, those living in an AFDC household were substantially less likely to have worked than those who were not (48% versus 63%). Sizable group differences were also observed in Boston and Phoenix relative to Hartford and San Antonio, respectively.

Finally, Table 6.5 presents adjusted percentages of teens residing in an AFDC household at the two follow-up interviews. At 12 months post-baseline, the percentage of teens living in a household in which at least one person received AFDC was the same in both groups. At 24 months, however, experimental teens were more likely to be living in an AFDC household. However, this difference partially reflected AFDC receipt by other household members; the difference in the percent of teens in the two groups who were receiving their own AFDC grant at the end of the study was small and nonsignificant.

As in the previous two chapters, information concerning the teens' activities relative to the date their index pregnancy was terminated was also analyzed. Table 6.10 shows that experimental/comparison group differences for the aggregated sample at 12, 18, and 24 months postpartum were small and nonsignificant.

Consistent with the findings reported in earlier chapters, when the dichotomous group variable was replaced with more specific indicators of program participation in the various employment-related analyses, program

TABLE 6.10

ADJUSTED PERCENTAGES OF TEENS EMPLOYED 12, 18, AND 24 MONTHS POSTPARTUM,[a] BY EXPERIMENTAL AND COMPARISON GROUPS[b]

Employment Variable	Adjusted Means, by Group		
	Experimental	Comparison	Difference
A. Entire Sample[c]			
Percent Employed 12 Months Postpartum (N=673)	13	15	- 2
Percent Employed 18 Months Postpartum (N=589)	18	18	0
Percent Employed 24 Months Postpartum (N=345)	18	20	- 2
B. Sample for Whom 24-Month Postpartum Data Were Available[c]			
Percent Employed 12 Months Postpartum (N=345)	11	15	- 4
Percent Employed 18 Months Postpartum (N=345)	18	21	- 3
Percent Employed 24 Months Postpartum (N=345)	18	20	- 2

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline and 24 months after baseline.

NOTES: [a]Postpartum refers to the period following the index pregnancy. The index pregnancy was either the pregnancy in progress at the time of the baseline interview or the most recently terminated pregnancy at baseline.

[b]The percentages in this table have been adjusted for ethnicity, school status at baseline, employment status at baseline, number of baseline jobs, and age of index child. The full regression table for 12 and 24 month outcomes are presented in Tables E.28 and E.29 of Appendix E. The regression table for 18-month employment has been excluded because the overall model was nonsignificant.

[c]Panel A presents data for every teen for whom school status information was available at the specified interval. Since many teens were pregnant at baseline, 24 months following the pregnancy had not yet elapsed at the time of the final interview for these teens. Panel B presents data for a constant cohort for whom data were available at all three intervals.

impacts were generally strengthened. For example, the average increment in the total number of jobs held associated with program participation was .26, as shown in Table 6.5. For teens who obtained job or employability training through Project Redirection, the average increase in jobs ever held was .44. Longer periods of program enrollment were also associated with better outcomes relative to comparison teens. Below are some figures showing differences between comparison teens and the experimental teens who were enrolled in Project Redirection for at least one year, after adjusting for important baseline characteristics (all were statistically significant):

	Experimentals Enrolled $\geq$ 12 Months	Comparison Teens	Difference
• Percent Either in School/ Completed or Employed at 24-Month Interview	58%	48%	10%
• Percent Either in School/ Completed or in the Labor Force at 24-Month Interview	79%	65%	14%
• Mean Number of Jobs Ever Held	2.38	1.90	.48
• Percent Worked Between Baseline and 24-Month Interview	69%	54%	15%

As before, although these statistics are encouraging and suggestive, they must be interpreted cautiously because of the possibility of self-selection factors.

Further analyses were conducted to examine the employment experiences of teens who either had or had not had a subsequent pregnancy during the study. The results, shown in Table 6.11, are similar to educational results presented for these subgroups in the previous chapter. For the aggregated sample, teens who had a subsequent pregnancy had substantially less positive employment outcomes than those who avoided an early repeat

TABLE 6.11

SELECTED EMPLOYMENT-RELATED OUTCOMES AT THE 24-MONTH INTERVIEW, FOR TEENS WITH OR WITHOUT A SUBSEQUENT PREGNANCY[a,b]

Selected Outcome	Adjusted Mean or Percentage, by Group			
	Teens With a Subsequent Pregnancy		Teens Without a Subsequent Pregnancy	
	Experimental	Comparison	Experimental	Comparison
Percent Either in School/Completed or Employed at 24-Month Interview	44*	34	55	59
Percent Either in School/Completed or in the Labor Force[c]	65**	51	81	76
Percent Employed at 24-Month Interview	10	6	21	21
Mean Number of Jobs Ever Held	2.17*	1.80	2.31+	1.98
Percent Ever Employed Post-Baseline	58*	46	66+	57
Number of Respondents	149	163	154	201

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] The figures in this table have been adjusted for relevant baseline characteristics; different covariates were used for different outcomes.

[b] "Subsequent pregnancy" refers to any pregnancy after the index pregnancy. The index pregnancy was either the pregnancy in progress at the time of the baseline interview or the most recently terminated pregnancy at baseline.

[c] A teen was considered to be in the labor force if she was currently employed or reported that she was seeking employment.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.

pregnancy. However, participation in Project Redirection helped to diminish the negative effects of a higher-order pregnancy. For example, only one third of the comparison teens but 44% of the experimentals with pregnancy. However, participation in Project Redirection helped to another pregnancy were either working or had a positive school status at the final interview. The difference was even greater when teens looking for work were included. Regardless of the teens' repeat pregnancy status at the final interview, participation in Project Redirection was associated with a higher mean number of jobs ever held and a higher rate of post-baseline employment, net of other factors.

In summary, the analyses in this chapter suggested that the program had long-term effects on teens' behaviors when schooling and employment were analyzed jointly. If one considers that either being in school, having completed school, or acquiring employment experience are all activities that can promote eventual self-sufficiency, then Project Redirection appears to have had some modest effects on moving teens toward this goal. Teens who had no baseline work experience or who were school dropouts at baseline or who had a post-baseline pregnancy were especially likely to sustain advantages. For the sample as a whole, 14% more of the experimental than the comparison teens were either in school, had completed school, were employed, or were looking for a job at the final interview.

In general, the employment impacts were similar in pattern to educational impacts. Participation in the program was not associated with higher rates of actual employment at the end of the study. However, cumulative measures of employment did indicate program effects. During the course of the two years, significantly more experimental than comparison group members had been employed. On average, experimental teens had accumulated significantly more work experience in terms of the number of jobs held. Given the generally poor economic conditions that prevailed during the course of the study, these gains seem especially noteworthy.

Even more noteworthy, however, were the observed experimental advantages for several key subgroups. For example, more of the experimental than comparison teens with no work experience at baseline obtained their first job during the two-year period. Teens with a subsequent pregnancy were more likely to have worked post-baseline if they had been enrolled in Project Redirection. Teens living in an AFDC household at baseline also profited from exposure to the program: 30% more experimental than comparison teens from an AFDC household had worked for pay during the two years under study. For this subgroup, program impacts were sustained beyond the teens' period of enrollment: at the 24-month interview, 60% more experimental than comparison teens were actually employed.

Program impacts in the employment arena were also found to be more substantial for teens with longer program enrollment and for those who obtained services relating to employment and the world of work. Teens enrolled in the program for more than one year were especially likely to have sustained long-term program impacts. It is not clear whether such effects represent primarily individual motivational factors or intensive exposure to program services and messages.

C. Redirection Impacts on Employability Measures

As indicated earlier in this chapter, several paper-and-pencil scales relating to job readiness and employment attitudes were administered to teens in this sample. These measures were included precisely because the youth of these teens was expected to limit full-time or long-term employment during the study period. Future employment and self-sufficiency were hypothesized to be affected not only by early work experience but by the teens' knowledge about the world of work and their orientation toward work and career. This section examines program impacts at 24 months post-baseline on these employability measures. Because the baseline scores on these measures were considered critical covariates, the results are presented for Sample I teens only.

Table 6.12 shows that, after adjusting for baseline scores and for other baseline characteristics, experimental teens scored significantly higher than comparison teens on the Employability Knowledge Test at the final interview. The experimental teens' average score was about a full point higher than that for both comparison teens and the similarly-aged standardization sample for the instrument.[4]

With respect to the Career Maturity Scale, comparison group teens scored slightly higher on the final administration than experimental teens, but the difference was not significant. As shown in Table E.31, receipt of employment-related training from Project Redirection (or

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[4] Sample II experimental teens also scored significantly higher than comparison teens on the Employability Knowledge Test after adjusting various baseline characteristics, with adjusted mean scores of 12.13 and 11.15, respectively. However, this result should be interpreted cautiously since it was not possible to adjust baseline scores on the Employability Knowledge Test.

TABLE 6.12

ADJUSTED MEAN SCORES ON EMPLOYABILITY MEASURES FOR SAMPLE I TEENS AT 24-MONTH INTERVIEW, BY EXPERIMENTAL AND COMPARISON GROUPS

Scale	Adjusted Means, by Group		
	Experimental	Comparison	Difference
Mean Score, Employability Knowledge Test	12.83	11.80	1.03***
Mean Score, Career Maturity Test	20.12	20.33	- .21
Mean Score, Attitude Toward Non-traditional Work Scale	14.35	14.38	- .03
Mean Score, Self-Esteem Scale	19.76	19.17	0.59*
Mean Score, Locus of Control Scale	14.89	14.42	0.47+

SOURCE: Tabulations are from AIR interviews with 166 experimental and 207 comparison group members in Sample I only at baseline and 24 months after baseline.

NOTES: The means in this table have all been adjusted for ethnicity, school status at baseline, and the baseline scores on the relevant scale. Various other characteristics were also controlled, but different covariates were required for different outcomes. The regression tables are presented in Tables E.30 to E.34 of Appendix E. All covariates significantly related to the outcomes, as shown in these appendix tables, were controlled in deriving the figures presented in this table.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
***Statistically significant at the .001 level.

another source) did result in an average increase of about one point in Career Maturity scores.[5]

Table 6.12 shows that experimental and comparison teens reported similar attitudes toward nontraditional employment for women at the 24-month interview. For both groups the average scores tend to reflect neutral attitudes toward employment that is not traditionally performed by women.

Two other scales were administered to Sample I respondents at baseline and follow-up. Although these scales are not directly related to employability, they are presumed to influence the teens' confidence in achieving established goals. The first is a six-item self-esteem scale that was adapted from the longer Self-Esteem Scale of Rosenberg. Scores on the abridged scale could range from six for the lowest levels of self esteem to 24 for the highest levels. After controlling baseline scores on this scale and other background characteristics, experimental teens had higher self-esteem scores at the end of the study than comparison teens.

The other psychological variable was locus of control, measured by a five-item scale. Locus of control is a widely-used construct that relates to a person's perceived sense of personal efficacy or control over life events. Those with an internal locus of control tend to believe that they themselves can control their own outcomes, while those with an external orientation view persons or circumstances external to them as the primary forces influencing those outcomes. The five-item scale used in this study had values that could range from five (the most externally-oriented score)to 20 (the most internally-oriented score). Table 6.12 indicates that, after adjusting baseline scores and other covariates, Project Redirection teens scored modestly but significantly higher than comparison teens at the final interview.

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[5] Sample II experimental teens also scored significantly higher than comparison teens on the Career Maturity Scale after adjusting various baseline characteristics, with adjusted mean scores of 20.52 and 19.38, respectively. As mentioned above, this result does not adjust for initial differences in Career Maturity scores and should be interpreted with care.

In summary, participation in Project Redirection was associated with gains on several measures of cognitive/affective development. Teens in the experimental group knew significantly more than teens in the comparison group about such matters as completing a job application and reading the details of a want ad by the end of the study, even after pre-treatment knowledge was statistically controlled. It was also found that program participation was associated with modest gains in the teens' self concept and in their beliefs that they could control their own lives.

CHAPTER 7

LESSONS OF PROJECT REDIRECTION

This chapter serves several purposes. First, it briefly discusses the results of the impact analysis in the context of the methodological issues raised in Chapters 2 and 3. Second, it considers comparative information from other data sets. The chapter concludes by offering some interpretations and implications of the findings.

A. Discussion of the Impact Analysis Results

The major purpose of this study was to assess the impacts of Project Redirection on the lives of young mothers from economically disadvantaged backgrounds. The underlying question was whether these teens' lives were improved by the program relative to what their lives would have been had they never received the program services.

The impact analysis design compared teens who participated in the Redirection program with teens presumed to be similar in every way except for the receipt of extensive, coordinated services. The intent of the design was to simulate as closely as possible a randomized experiment by using statistical techniques to adjust for minor group differences that were expected to occur. This intent was not realized for two reasons. Comparison teens were more different from experimental teens than expected on several important background variables and less different in terms of service receipt.

Project Redirection was designed to provide or broker comprehensive services to pregnant or parenting teens through a coordinated, supportive approach. Chapter 3 provided evidence that this objective was achieved. However, Chapter 3 revealed as well that the comparison group was also receiving a broad range of services, with 54% enrolled in special programs for assisting teen mothers. It was never expected that the comparison group would not be served, given its selection through contact with service providers. However, it was also not expected that the level of service receipt--while lower than than of program participants--would approach that for teens in the programs funded by the federal Office of Adolescent Pregnancy Programs.

Given the high level of service receipt in the comparison group, the impact analysis cannot address the question of whether or not Redirection's services were effective (compared to an absence of services) in improving the teens' long-term outcomes in the areas of fertility, schooling and employment. It addresses instead the incremental effectiveness of offering comprehensive services in a particularly supportive milieu. That milieu included assistance and encouragement from the community women, coordination and support from staff who recognized the importance of developing self-esteem, and opportunities to receive support from and interaction with peers in a caring environment.

It should also be noted that, as a comparison of alternative modes of intervention, the implemented design was relatively insensitive. Sample size estimates had been originally developed assuming that comparison teens would receive only fragmented, limited services. To detect the small incremental effects of the more intangible support system that was particular to Project Redirection at statistically significant levels, much larger sample sizes would have been required.

The second methodological concern is self-selection bias. Although efforts were made to reduce this risk, the design was less successful than hoped. The comparison group may have been a more motivated group than a random sample of similar teens, and even more motivated than the teens who elected to participate in Project Redirection. Two major types of evidence support this inference:

(1) Comparison teens had to volunteer to be interviewed initially and had to cooperate again for the follow-up interviews; only a small monetary incentive was offered in exchange for their cooperation. Teens willing to do this are probably more motivated than average, and probably above average in the orderliness of their personal lives. The very high follow-up completion rate among comparison teens substantiates this view; and

(2) About 40% more comparison than experimental teens were enrolled in school at baseline. School enrollment is presumably strongly influenced by the teens' level of motivation or other favorable circumstances. Other measured school-related differences favoring comparison teens also suggested higher motivation. While baseline school enrollment was controlled in the analyses, it is possible that other unmeasured characteristics associated with school enrollment (e.g., motivation, ability, and situational factors) remained uncontrolled.

Taken as a whole, these indications suggest that this impact analysis was in fact a conservative test of Project Redirection's effectiveness. That is, in light of the comparison teens' service utitization, as well as their baseline school records and voluntary participation in the study, this evaluation probably reflects the lower bounds of Project Redirection's effectiveness.

Given this methodological context, it is difficult to judge the true longer-term impacts of the program model. The conservative nature of the design does not enable us, however, to conclude that Redirection participants did not benefit in the long run from enrollment in the program relative to what would have happened in the absence of services. Instead, the implemented design permits us to draw modest conclusions about the Redirection participants relative to comparison teens, as follows.

First, the data indicate that Project Redirection had many temporary incremental impacts on the teens' lives, mostly while they were in the program. At 12 months after baseline, experimental teens were significantly less likely to have had a repeat pregnancy, more likely to be in school, and more likely to have had a paying job in the previous year than comparison teens. These impacts were true both for the aggregated sample and for most subgroups of teens.

However, two years after enrollment, when teens were no longer in the program, most of these positive impacts disappeared. At the final interview, the behavior of Redirection teens was very similar to that of the comparison teens: they were just as likely to have had a repeat pregnancy, to have dropped out of school, and to be unemployed. An important exception was that experimental teens were more likely to be either in school (or to have completed school) or to be in the labor force. Experimental teens also scored better than comparison teens on several non-behavioral outcomes, such as tests of birth control knowledge, employability knowledge, self-esteem, and personal efficacy. By and large, however, the positive incremental impacts for the sample as a whole at one year after baseline had deteriorated by the final interview.

Despite the relatively few enduring effects for the sample as a whole, a third finding is that Project Redirection had lasting incremental impacts for certain subgroups of teens. Three subgroups, all of whom may be characterized as more disadvantaged than the average Redirection teen, deserve special mention: school dropouts at baseline, teens living in an AFDC household at baseline, and teens experiencing a pregnancy subsequent to the index pregnancy.

Table 7.1 summarizes the major 24-month outcomes for teens not enrolled in an educational program at baseline. Experimental dropouts were significantly more likely than their comparison counterparts to be either in school, working, or looking for work at the final interview. Project Redirection was an especially powerful influence in encouraging these teens to return to school. Nearly twice as many experimental as comparison teens had returned at some point, and they had spent nearly three times as many full semesters in school. Most importantly, nearly twice as many Project Redirection participants as comparison teens (20% versus 11%) had obtained a diploma or GED certificate by the end of the study. Since school dropouts are an especially difficult group to assist, this incremental impact is quite substantial. Within this subgroup, however, fertility-related outcomes at the end of the study were not affected by program participation at statistically significant levels.

Major 24-month outcomes for teens living in an AFDC household at baseline are summarized in Table 7.2. For this subgroup, longer-term incremental impacts cut across fertility, educational and employment areas. Among these teens, about one-third more comparison than experimental teens had a live birth within the two-year study period. The program also influenced the teens' school behavior, although this did not result in higher rates of school completion. In contrast, there were employment impacts at the time of the final interview: 60% more experimental than comparison teens (16% versus 10%) from an AFDC household were employed.

TABLE 7.1

SELECTED OUTCOMES AT FINAL INTERVIEW FOR TEENS NOT IN SCHOOL AT BASELINE

Outcome Variable	Adjusted Mean or Percentage, by Group			
	Experimental Teens	Comparison Teens	Difference	Percent Increase/ Decrease [a]
Percent with a Subsequent Pregnancy	56	58	- 2	- 3
Percent with a Subsequent Live Birth	32	41	- 9	-22
Percent in School/GED Program or Completed	28	18	10*	+56
Percent Obtained Diploma or GED Certificate	20	11	9*	+82
Percent Ever Enrolled in an Educational Program Post-Baseline	72	38	34***	+89
Mean Number of Full Semesters in an Educational Program Post-Baseline	1.45	0.61	0.84***	+138
Percent Either in School/Completed or Working	36	25	11*	15
Percent Either in School/Completed or in the Labor Force	62	46	16***	30

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: All means and percentages shown have been adjusted for relevant background characteristics.

[a] The percent increase or decrease was calculated by dividing the experimental/comparison group difference by the comparison percentage (or mean).

*Statistically significant at the .05 level.
***Statistically significant at the .001 level.

TABLE 7.2

SELECTED OUTCOMES AT FINAL INTERVIEW FOR TEENS IN AN AFDC HOUSEHOLD AT BASELINE

Outcome Variable	Adjusted Mean or Percentage, by Group			
	Experimental Teens	Comparison Teens	Difference	Percent Increase/ Decrease[a]
Percent with a Subsequent Pregnancy	44	52	- 8	-15
Percent With a Subsequent Live Birth	21	32	-11*	-34
Percent in School/GED Program or Completed	40	42	- 2	- 5
Percent Obtained Diploma or GED Certificate	19	17	2	12
Percent Ever Enrolled School Post Baseline	86	70	16***	+23
Mean Number of Semesters in School Post-Baseline	2.01	1.53	0.48***	+31
Percent Either in School/Completed or Working	48	44	4	9
Percent Employed	16	10	6*	+60
Percent Ever Worked, Post Baseline	63	48	15**	+31

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: All means and percentages shown have been adjusted for relevant background characteristics.

[a] The percent increase or decrease was calculated by dividing the experimental/comparison group difference by the comparison percentage (or mean).

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

The third subgroup for whom longer-term program impacts were observed were the teens who became pregnant again during the study period (Table 7.3). Compared to experimental teens, comparison teens with a subsequent pregnancy had especially poor educational and employment outcomes. It thus appears that Project Redirection participation helped to minimize the negative consequences associated with an early repeat pregnancy. An especially noteworthy finding was that two-thirds more of the experimental teens (20%) than comparison group teens (12%) had completed their basic schooling by the end of the study.

Thus, while this study could not clearly assess whether Project Redirection produced longer-term impacts on its participants' lives relative to no program, it appears that it did have some incremental short-term effects, and--among some subgroups of especially disadvantaged teens--some incremental longer-term impacts relative to a well-served comparison group. Additionally, it was found that several educational and employment-related outcomes were sustained for the entire 24-month period for teens who remained in the program for at least 12 months and for those who received specially targeted program services. Although longer program stay and receipt of specific services could reflect motivational and situational differences, these differences were presumably at least partially controlled statistically, suggesting that sustained, targeted intervention of the Redirection type can have some lasting effects on the lives of disadvantaged teens.

Nevertheless, the results of this study are not overly encouraging. Regardless of any observed incremental gains and the ambiguity introduced by the methodological problems, the absolute level of continuing disadvantage to teens in both groups suggests that current models of

TABLE 7.3

SELECTED OUTCOMES AT FINAL INTERVIEW FOR TEENS WITH A SUBSEQUENT (POST-BASELINE) PREGNANCY

	Adjusted Mean or Percentage, by Group			
Outcome Variable	Experimental Teens	Comparison Teens	Difference	Percent Increase/ Decrease [a]
Percent in School/GED Program or Completed	41	31	10+	32
Percent Obtained a Diploma or GED Certificate	20	12	8+	+67
Percent Ever Enrolled in School Post Baseline	86	63	23***	+37
Mean Number of Semesters Enrolled in School Post Baseline	1.94	1.30	0.64***	+49
Percent Either in School/Completed or Working	44	34	10*	15
Percent Either in School/Completed or in the Labor Force	65	51	14**	29
Percent Ever Worked Post Baseline	58	46	12*	26

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: All means and percentages shown have been adjusted for relevant background characteristics.

[a] The percent increase or decrease was calculated by dividing the experimental/comparison group difference by the comparison percentage (or mean).

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

intervention are not adequate. By the end of the study, nearly half of the teenagers in both groups had given birth or were soon to give birth to a second (in some cases, a third) child. Among teens who were 18 years or older at the final interview, only 25% had a diploma or GED certificate. These findings pose the most serious challenge for new directions.

B. Additional Perspectives

Because the comparison group strategy used in this study resulted in various interpretive problems, additional steps were taken to understand the experimental group outcomes. Primarily, an extensive review of published and unpublished documents on teenage parents was conducted to develop additional estimates of outcomes for "typical" teen mothers. For the purpose of direct comparisons, the major outcomes of this study were adjusted to reflect behaviors at fixed points relative to the date of pregnancy termination (in lieu of using the baseline date as a reference point).

Although dozens of studies and evaluations were reviewed, the effort yielded disappointingly few meaningful results. Two problems were evident. First, the evaluation studies were usually not considered reliable because of small samples, lack of (or poor) comparison groups, or high attrition from the sample. The second problem was that sampling criteria were so different that direct comparisons were generally impossible. Project Redirection teens were younger and more economically disadvantaged than almost all other samples of teen parents, characteristics that would tend to depress the rate of favorable outcomes such as school completion. Appendix D summarizes in greater detail the results of this review.

Another procedure was thus explored for obtaining estimates of outcomes from a general population of teen parents. This would have involved a secondary analysis of data from a national sample of young people. With such an analysis,the sample would have been selected using the same criteria used for enrolling teens into the Project Redirection programs. It was assumed that teens from a general population would be substantially less well served than the teens in Redirection's comparison group.

The most appropriate data set was the National Longitudinal Survey of Labor Market Experiences of Youth (NLS), a longitudinal survey of over 12,000 young men and women who, in the first year of the survey (1979), were between the ages of 14 and 21. The NLS survey drew a national sample, with an overrepresentation of blacks, Hispanics and economically disadvantaged whites. However, only 176 cases met the Project Redirection eligibility criteria, and some of these teens lived in rural areas. Moreover, the survey did not collect data on service receipt. Consequently, the secondary analyses were not undertaken. The small NLS subsample that met Redirection's entry criteria underscores the fact that this program is working with a highly distinct, select subset of American youths—a subset with which policy makers at all levels are becoming increasingly concerned.

In one more effort to shed some light on what a "typical" teen parent's life might be like in the absence of special programs, the comparison group was divided into two subgroups: teens who had ever participated in a special program for teen parents (N = 204) and teens who had not (N = 167). These two groups were then compared with short-term and long-term enrollees in Project Redirection. As before, it must be pointed out that the results must be interpreted cautiously because differences in outcomes could reflect differences in individual characteristics (such as motivation or competence) that lead teens to use services, or differences resulting from actual service receipt. However, important background characteristics such as age at first birth, ethnicity, baseline school status and baseline employment experience were statistically controlled in an attempt to eliminate major initial differences between the four groups.

The results, summarized in Table 7.4, indicated that for virtually every outcome examined, comparison group teens who had never enrolled in a special program had the least favorable outcomes at the final interview, while teens enrolled in Project Redirection for at least 12 months had the most favorable outcomes. These analyses suggest that the "typical" teenage mother--who is generally receiving routine, but uncoordinated services from a variety of social service and health agencies--has a substantially more difficult time in the areas of employment and schooling than her peers who enroll in specially designed programs. They are also consistent with an interpretation that Project Redirection, a program unusually successful in retaining teens for more than one year, had lasting impacts on long-term enrollees. The above results suggest a continuum of outcomes, with those receiving the poorest service delivery package having the poorest record and those with the best service package performing the best. While other conclusions may be inferred from these findings, this interpretation seems sensible and deserving of consideration.

TABLE 7.4

ADJUSTED 24-MONTH OUTCOMES FOR FOUR SERVICE-DEFINED GROUPS

Outcome	Comparison		Experimental		
	Never in A Teen Parent Pgm.	Ever in A Teen Parent Pgm.	Enrolled < 12 Months	Enrolled ≥ 12 Months	
Percent with a Subsequent Pregnancy	51	47	49	40	$p > .10$
Percent with a Subsequent Live Birth	32	27	25	19	$p < .10$
Percent Completed School/GED Program	19	20	17	27	$p < .10$
Percent Ever in School, Baseline to 24-Month Interview	59	78	84	94	$p < .001$
Mean Number of Semesters in School Post-Baseline	1.35	1.69	1.82	2.48	$p < .001$
Percent Either in School/Completed or Employed	45	49	46	58	$p < .10$
Percent Either in School/Completed or in the Labor Force	62	67	71	79	$p < .01$
Percent Ever Worked Post-Baseline	50	57	59	69	$p < .01$
Mean Number of Jobs Ever Held	1.96	1.82	2.10	2.40	$p < .05$
Number of Respondents	167	204	172	129	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: All means and percentages have been adjusted for relevant background characteristics.

C. Implications of the Findings

While the nature of the design implemented in this study does not permit conclusions about which features of Project Redirection might have been more successful than others, nevertheless, some overall inferences can be made about the program model. First, the subgroup findings for longer-term outcomes suggest the desirability of targeting intense, coordinated programs of the Redirection variety to teens who have the fewest assets initially. The results of this study suggest that teens entering the program with more assets may do as well with services that are generally available.

The subgroup analyses also suggest what dimensions do not make a difference. The dearth of differential age and parity subgroup effects is particularly noteworthy. When positive program effects were observed for the aggregated sample, virtually all subgroups benefited. When overall effects were not found, there were no subgroups that seemed adversely affected. This suggests that Redirection-type programs can be effectively designed to simultaneously accommodate the needs of older and younger teens as well as pregnant teens and teen mothers.

The finding that the program had powerful incremental effects in the interim period that largely disappeared after two years poses a challenge for the design of services with longer-term beneficial outcomes. The critical question is what influenced the decay of program impacts. The most plausible answer seems to be the teens' departure from the program. Between baseline and the 12-month interview, when all experimental teens were enrolled, they accrued impressive gains in every sphere examined, even in comparison with a sample of other well-served teens. It is thus likely that, once these teens left the program and continued in their old environments[1], perhaps without contact with other sources of service provision, the beneficial effects of the program disappeared. In some cases experimental teens may have been worse off in terms of support service access in the last year of the study than their comparison counterparts.[2]

The explanation that departure from the program resulted in deteriorating impacts is consistent with the finding that teens enrolled in Project Redirection for at least one year did sustain positive long-term impacts. It has repeatedly been noted that this finding could reflect greater program inputs or greater motivation on the teens' part in seeking out those services or activities.[3] However, even granted that it takes some motivation to continue participation, it may not be realistic to conclude that these teens would have done as well "anyway."

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[1] It is important to remember that, although there were varying degrees of disadvantage among teens in this sample, all of these teens came from backgrounds that make their long-term prospects for economic advancement bleak in the absence of some "redirecting" force. Their families are poor. Most teens were raised in single-parent families with numerous siblings. Their mothers generally had dropped out of school and had been teen mothers themselves. Responses to several open-ended questions suggested that many of these teens lived in environments in which conflict and crisis were common: reports of physical violence, emotional trauma, alcoholism, drug abuse, and incarceration among family members or the teens' partners were disturbingly frequent.

[2] Another factor influencing deteriorating program impacts might be the relative increases in service provision to comparison teens during the final study year. There is some modest evidence that this occurred, but it seems unlikely that this factor accounted for the major shift in impacts between the 12- and 24-month interviews, although it may have contributed to such a shift.

[3] In a sophisticated analysis that controlled for selectivity in analyzing the effects of length of program stay on outcomes among participants in the National Supported Work Program, it was found that longer enrollments did have positive impacts on many employment outcomes. The Supported Work program was targeted, in part, at AFDC mothers (Masters and Maynard, 1981, Appendix C).

Given these teens' backgrounds and their responsibilities as parents of infants, it is unlikely that motivation alone could result in favorable outcomes. It seems more reasonable to conclude that Project Redirection provided the teens with the resources that made it possible to convert motivation into performance. Furthermore, the program itself could have generated some of that motivation.

It seems sensible to expect that longer programs can have more impact on a person's life than short-term ones. A mandatory curtailment of services after, say, 12 or 18 months suggests that the need for services no longer exists. This is certainly not the case for the teens in this sample, nor is a recommendation for sustained services to this group a new idea. Furstenberg (1976), who referred to most teen parent programs as a weak "inoculation," made the following observation nearly a decade ago:

> Most programs for the adolescent parent are based on the premise that short-term assistance will have a long-term impact. We have discovered that short-term services produce short-term effects. If we are to have any hope of influencing the career directions of adolescent parents, it is not enough to be present when plans are formed at important junctures in the life course; we must be available to ensure that these aims are implemented (p. 163).

Several possible approaches could be taken to extend program services for a longer period. The first, and most obvious, is simply to remove mandatory exit criteria based on age or time spent in the program. This would ensure that services were provided to all teens who felt they still could benefit from continued participation. Additionally, since many of the teens in Project Redirection and other similar programs terminate voluntarily, it might be desirable to consider some incentives for longer participation. Redirection teens were initially given a small monthly stipend. It may prove more useful to reward longevity, or incremental stipends could be associated with either goal accomplishments or length of program stay.

Another strategy could emphasize "booster shots" for the teens. That is, rather than stressing continuous enrollment, staff could offer teens the option of returning periodically for short-term, targeted services either on a fixed-time schedule (e.g. every six months) or at intervals coinciding with crises in the teens' lives. In either case, aggressive outreach and follow-up would be required to maintain contact with the teens and to stimulate their interest and motivation.

Finally, an approach requiring fewer resources could rely on regular, ongoing telephone contact (or short home visits) to the teens. For programs like Redirection, such contact could be initiated either by program staff or the community woman. These follow-up contacts could offer counseling, support, or referral to other available services. There may be still other possible strategies but somehow, monitoring these teens in an ongoing fashion to keep track of any possible slippage seems particularly important.

Thus far two ways of building on the lessons of Project Redirection--targeting the program carefully and extending services or contact with the program--have been discussed. A final consideration is how the program itself could be strengthened.

The data from this study suggest that, given the high rate of repeat pregnancies, the programmatic area most in need of strengthening is the family planning component. Some argue that a delay in subsequent pregnancies is among the most critical factor in judging the program's sucess in "redirecting" teens' lives. Yet it must be recognized that this outcome is probably the most difficult to affect; it involves familial, societal and peer forces that are often not within the sphere of the program's influence. Program staff and community women can work more directly with the teens on other goals such as education or jobs.

Getting sexually active teens to consistently use effective contraception has been a problem that has defied the concerted efforts made to resolve it. Nevertheless, even fairly routine birth control counseling may have some effect. The results of this study indicated, for example, that teens who received contraceptive counseling had significantly more experience with medically prescribed contraceptives and fewer pregnancies at the end of the study. According to the teens' self-reports, most participants (74%) did receive birth control counseling within 12 months of enrollment. The question is, however, why not all teens did. Furthermore, among teens enrolled more than one year, only 45% said they received birth control counseling from any source during the last year of the study, and just 12% said they received it directly from the program. Given these teens' known sexual experience and their clear need for birth control, contraceptive counseling should be a top program priority, addressed on an ongoing basis.

In conclusion, the findings of this impact analysis study suggest several avenues for programs directed to young mothers. An encouraging sign from the analysis presented in the previous section of this chapter is that special services for teen parents may indeed improve the educational and work-related experiences of these teens after consistent long-term intervention. However, the poor performance of both experimental and comparison teens on all outcomes considered critical to future self-sufficiency--even after program intervention for many teens--provide a clear message: "redirecting" these teens' lives is a challenge of enormous proportions. No matter what the program accomplished, it was not enough. The difficult task remaining is to design stronger, more effective treatments that can serve more as a true opportunity for the teens in reshaping their lives.

APPENDIX A

ISSUES RELATING TO SAMPLE I AND SAMPLE II

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Introduction

The Project Redirection Impact Analysis began with a sample of teens enrolled in the program sites between September 1980 and March 1981 (July 1981 for Riverside), together with their comparison group counterparts; collectively, these subjects comprise Sample I. All of these teens were administered a baseline interview, and were re- interviewed 12 and 24 months later. In the fall of 1981, funds became available to enlarge the research sample. Teens enrolled in the program between March 1981 and March 1982, together with a group of comparison teens, comprise Sample II of the Impact Analysis. The difficulty with this second sample is that they were not administered a baseline interview, inasmuch as the decision to enlarge the sample came after many Sample II teens were already enrolled in the program.

Because Sample II teens were not baselined, various methodological issues needed to be addressed in handling the Sample II data. Two issues of primary concern were the absence of missing data and the comparability of data from Samples I and II. This appendix summarizes those issues and describes strategies employed to deal with them.

Absence of Baseline Data

Without baseline data, the pre-treatment equivalence of an experimental and comparison group is difficult to assess. In such a situation, the problem of selection bias poses serious threats to the internal validity of a study. Furthermore, without baseline data, the analytic techniques generally used to control pre-treatment group differences would not be feasible. Therefore, a major concern in this study was to develop a strategy to deal with the absence of baseline data for Sample II.

Inspection of the interim analyses of impacts for Sample I alone (Polit *et al.*, 1983) suggested that baseline covariates used to model program outcomes could be classified into one of three categories: (1) "constants" that would not be expected to change from baseline to the 12-month interview; (2) variables for which retrospective information was obtained from Sample II respondents at follow-up, such that baseline status could be reconstructed; and (3) variables for which no retrospective data could meaningfully be obtained in the "12-month" interview. Table A.1 lists variables included in the Sample I impact analyses according to these three categories.

The first category ("Constants") consists of variables that are presumably unrelated to the time of the interview (e.g. mother's education). The reliability and validity of self-report data would be expected to be the same regardless of when data for these variables were collected. Therefore, data from the 12-month follow-up interview for covariates in this category were used.

The next category poses more problems. In the baseline interviews for Sample I, respondents were asked a number of questions regarding their

TABLE A.1

BASELINE VARIABLES USED AS COVARIATES IN SAMPLE I ANALYSES, BY TYPE

- **Group 1 -- Constants**

Birthdate
Ethnicity
Mother's education
Father's education
Mother's age at first birth

- **Group II -- Retrospective Variables**

In school at baseline
Left school because of a pregnancy
Amount of time out of school at baseline
Highest grade completed
Number of semesters skipped
Number of semesters repeated
Number of semesters not in school
Employed at baseline
Number of jobs ever held
Any work experience prior to baseline
Marital status
Pregnant at baseline
Number of pregnancies
Date of termination of pregnancies
Ever used contraceptives

- **Group III -- Missing Variables With No Retrospective Data**

Planning to return to school (baseline dropouts)
Absentee rate from school
Educational aspirations
In a vocational/business curriculum
Career Maturity scores
Employment Knowledge Test scores
Birth Control Knowledge Test scores
Mother present
Father present
Husband/boyfriend present
Household income
Sexually active

educational, employment, pregnancy, and contraceptive status, and responses to these questions were then used as control variables in the impact analyses. In the Sample II 12-month interviews (as well as the Sample I 12-month interviews) detailed histories were obtained for the teens' schooling back to grade one, all employment experiences, and all pregnancies. From these retrospective histories, baseline information could be reconstructed. This information could, then, be used to represent missing baseline data. The question that arose, however, was whether the retrospective data were sufficiently reliable to warrant inclusion as covariates.

Fortunately, it was possible to estimate the reliability of some of the Sample II retrospective data based on data from Sample I. For the first sample, both baseline information and retrospective histories were obtained. Thus, it was possible to compare "real" baseline data with reconstructed baseline data. Since there appears to be little reason to suspect that the reliability of retrospective data would be different for Samples I and II, it was concluded that this analysis would provide a reasonable estimate of the accuracy of Sample II retrospective data.

Another issue, however, was the reliability of the actual baseline data itself. Assessments of the reliability of self-report survey data are seldom easy and rarely attempted. One notable and relevant exception was the 1971 Survey of Young Women by Kantner and Zelnik. In this survey, teenagers aged 15 to 19 were interviewed regarding sexual behavior, contraception and pregnancy. Approximately 10 percent of the sample was re-interviewed with an abbreviated schedule one to four months after the initial interview. The response consistency for the sample as a whole and for individual respondents was then examined (Zelnik, Kantner and Ford, 1981, Appendix B). Since the Kantner and Zelnik survey focused on similar and sensitive topics with a same-aged sample, their reliability estimates provide a base against which to compare reliability estimates for retrospective reports. In other words, Sample II baseline data, even if it had been collected, would not be perfectly reliable; consequently, agreement between the Sample I baseline and retrospective reports will inevitably fall short of 100 percent.

Five variables were selected to study consistency of responses at baseline and 12 months post-baseline among Sample I respondents: (1) in school/not in school at baseline; (2) ever worked/never worked at baseline; (3) number of jobs held at baseline; (4) pregnant/not pregnant at baseline; and (5) number of pregnancies at baseline. These variables were selected to represent key "outcomes" variables in the three main areas of the impact analysis.

Table A.2 shows the percent of agreement between actual baseline responses and responses coded from the follow-up retrospective histories for these five variables, broken down by site. Several observations regarding this table are in order. First, levels of agreement are reasonably high for all variables and all sites. Second, pregnancy variables have the highest rate of agreement. Number of jobs held had the lowest rate, and this is attributable primarily to discrepancies in reports about babysitting. Third, there do not appear to be any systematic site or group differences in levels of agreement. Sites with exceptionally high (or low) levels of agreement on one variable did not have uniformly high (or low) rates on other variables. The experimental

TABLE A.2

PERCENT AGREEMENT ON FIVE BASELINE VARIABLES
MEASURED AT BASELINE AND 12 MONTHS IN SAMPLE I, BY SITE

ite	School Status at Baseline	Ever Worked at Baseline	Number of Jobs Held at Baseline	Pregnancy Status at Baseline	Number of Pregnancies at Baseline
oston	77.8%	88.9%	85.2%	96.3%	96.3%
artford	88.9	83.3	83.8	96.8	100.0
arlem	92.5	97.5	80.0	100.0	100.0
ed-Stuy	93.0	93.0	78.9	100.0	94.7
hoenix	93.8	87.8	70.7	98.8	100.0
an Antonio	98.9	90.9	78.4	97.7	95.5
iverside	93.5	94.4	83.3	100.0	100.0
resno	89.5	89.7	79.5	100.0	97.4
OTAL	92.8%	90.8%	78.5%	98.8%	97.4%

SOURCE: Tabulations are from AIR baseline and 12-month interviews with Sample I respondents.

and comparison groups do not appear to be markedly different from each other in terms of consistency: the "average" percent agreement for the two groups, averaged across sites and variables, was 91.3 percent for the experimental group and 91.5 percent for the comparison group.

These figures compare favorably with the consistency levels reported in Kantner and Zelnik. Although none of the five variables listed in Table A.2 was exactly the same as those for which reliability was assessed by Kantner and Zelnik, the following examples (for black respondents) illustrate that levels of accuracy[1] are similar:

Age	94.6
Religion	92.5
Ever had a child	97.0
Desired number of children	67.5
Intend to have children	91.0
Ideal age of marriage	33.2

Kantner and Zelnik noted that consistency tended to be high for factual or behavioral variables and low for attitudinal or motivational variables. Their levels of agreement for factual data are generally in the low to mid 90's, not substantially higher than those obtained for the five variables in Table A.2. Indeed, the Kantner and Zelnik data, obtained from interviews administered one to four months apart and often based on identically-worded questions, suggest that the quality of the retrospective data for the present study is remarkably good.

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[1] Kantner and Zelnik converted raw levels of agreement with the formula $(0-(1/n))/(1-(1/n))$, where 0 is the observed proportion of agreement. With large samples sizes, the difference between the resulting value, shown above, and observed proportions, as shown in Table A.2, is marginal.

One further analysis was performed to further assess the appropriateness of using reconstructed baseline covariates for Sample II. Regression analyses were performed for Sample I in which reconstructed baseline covariates were substituted for actual baseline covariates. The results indicated that changing the predictors had little effect on the results (not shown in tables). Substituting reconstructed baseline covariates for actual information obtained at baseline resulted in only minor changes to regression coefficients, the levels of significance, and the overall R^2. On the basis of these analyses, it was concluded that it would be reasonable to use the retrospective histories obtained from Sample II at 12 months post-baseline to reconstruct factual baseline variables.

Variables listed in Group III of Table A.1 were more problematic. These are variables for which retrospective data were considered difficult or impossible to accurately obtain. For example, teens could not be expected to respond to a Birth Control Knowledge Test based on what they knew 12 months earlier. Similarly, it was considered improbable for teens to accurately remember and report what their educational ambitions had been a year before the first interview.

After considering various alternatives for handling missing data (deletion of variables; deletion of cases; replacement with mean values; and estimation by various methods), it was concluded that different strategies would be appropriate for different covariates. It was decided that the Group III variables would be treated as follows:

- Deletion of baseline variables (Planning to return to school, absentee rate from school, type of school curriculum, household income, and sexual activity)--These were covariates that were not significant in Sample I analyses and which, when removed from the model, resulted in virtually no changes.

- Deletion of cases (Educational aspirations, Career Maturity Scores, Employment Knowledge Test Scores, Birth Control Knowledge Test Scores)--These baseline covariates were extremely powerful predictors of follow-up measures of the same trait, so deletion of these variables would not be appropriate. Furthermore, reliable estimation of these baseline variables proved not to be feasible. Therefore, analyses relating to these traits measured at follow-up were restricted to Sample I cases.

- Estimation of values (Mother present at baseline, father present at baseline, husband/boyfriend present at baseline)--Although regression is often used to estimate missing values, this procedure was ruled out in the present situation because (1) missing values could not be assumed to be random; (2) many of the best predictors for certain covariates were other variables missing for Sample II cases; and (3) other predictors would also be used as predictors of outcomes, which would result in estimates colinear with the covariates. Therefore, the only estimated values were for variables that lent themselves to an assumption of continuity over time. For Sample I teens, if a parent or spouse was present at 12 months post-baseline, he or she was almost always present at baseline. Therefore, estimates of baseline household composition were made for Sample II based on household composition at the 12-month interview.

Comparability of Sample II Experimental and Comparison Groups at Baseline

Using reconstructed baseline variables, it was possible to examine the pre-treatment equivalence of the Sample II experimental and comparison teens. As reported in the text (Chapter 2), the two groups were well matched demographically. The most substantial group difference emerged in the educational area: nearly 50% more comparison than experimental teens were in school at baseline. The magnitude and direction of this difference was similar to that observed for Sample I. Site-level analyses indicated that virtually all sites (California being the exception) contributed to the educational differences.

Another disturbing difference was the amount of time in which teens were exposed to the risk of a subsequent pregnancy. The comparison teens were more likely than experimental teens to have been pregnant at baseline. As a result, the mean difference in the number of months elapsed between termination of the index pregnancy and the administration of 24-month interview was over four months for Sample II teens.

Site-level comparisons between the matched experimental and comparison pairs generally revealed modest differences, except for school status and at-risk period. In fact, at the site level the match was better for Sample II than for Sample I. It was concluded that analytic controls would be essential to reduce the measured baseline differences between Sample II experimental and comparison teens, but that the comparability of the two groups was sufficiently high to warrant further analyses.

Aggregation of Samples I and II

A significant issue was the question of whether Samples I and II could be pooled. If subjects from the two samples were similar (i.e., drawn from the same population), then it would be advantageous to aggregate the data for analytic purposes. Indeed, a primary reason for adding a second sample was to enlarge the sample size so that certain analyses (e.g., site-level impact analyses) would be possible.

There were several *a priori* reasons for believing that pooling would be justified. First, the two samples were drawn from the same sites, so that any environmental or external forces operating on outcomes (or the treatment) should be comparable. Second, eligibility criteria for program participation (and comparison group selection) were essentially the same.

Third, the definition of the two samples was not linked to any programmatic, fiscal, or administrative changes. If a teen enrolled in the Harlem program on March 31, 1981, then she was in Sample I; if the same girl enrolled the following day she was in Sample II. In other words, the distinction between the two samples was based exclusively on the research design. And fourth, analysis of interview data showed that the Sample I and II teens were similar demographically as well as with respect to important baseline behaviors.

On the other hand, while abrupt discontinuities coincident with the enrollment of Sample II teens could not be identified, it was recognized that changes over the 20-month baseline period were likely to have emerged. As one known example, general economic conditions deteriorated between 1980 and 1982. Changes in staffing, recruitment, programmatic emphases, termination criteria, and funding also occurred. Because of these time-related changes, it seemed reasonable to explore whether time dependencies should be modeled and, if so, whether an alternative to the dichotomous Sample I/Sample II distinction improved the modeling of time factors.

Four alternative methods of handling time dependencies (in addition to a Sample I/Sample II dichotomy) were examined using regression analyses in which various 12-month outcomes were used as the dependent variables:

- First, the sample was divided into three groups of approximately equal size, with each group covering a different baseline time period. The use of three groups was arbitrary, but no more arbitrary than the Sample I/Sample II distinction. Furthermore, the use of three groups resulted in the creation of a group that was approximately equally divided between Sample I and Sample II respondents. The three groups were "baselined" according to the following schedule:

  —TGROUP=1: July 9, 1980—February 5, 1981 (N=245);
  —TGROUP=2: February 6, 1981—June 20, 1981 (N=247);
  —TGROUP=3: June 21, 1981—March 30, 1982 (N=245).

- Second, eight dummy time variables (T1 to T8), each corresponding to a three-month period from June, 1980 to May, 1982 were created. These dummies, which may be viewed as approximating seasonal influences as well as changes over time, were entered as predictor variables in regression analyses for the aggregated sample.

- Third, a continuous time variable (ENROLDAY) was created. The date corresponding to the first date of enrollment was coded 1, with increments of one for each succeeding day. This variable was then entered as a predictor variable in the regressions for the entire sample.

- Fourth, to test for the existence of nonlinearities, the square of the continuous variable (ENROLDA2) was added to the regression analyses.

The results indicated a strong pattern of time dependencies for employment outcomes, but few consistent patterns for educational or fertility outcomes. In the employment arena, work experience declined systematically over the 20-month enrollment period, though the rate of decline leveled off in later months. No matter which procedure was used to model time factors, the results were essentially the same. It was concluded that it would be necessary to control for time dependencies in the analysis of 12-month employment outcomes.

The question that remained was whether the best way to control for these dependencies was to maintain the Sample I/Sample II distinction or to use one of the alternative methods. Table A.3 shows the results of four regression analyses in which employment experience between baseline and follow-up were regressed on background variables and alternative time-related variables.[2] In each regression, interaction terms were

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[2] Ordinary least-squares (OLS) regression was used, despite the dichotomous outcome variable, because the focus was on stability of results with different time variables. The R^2 provides a convenient summary statistic when OLS is used. The overall mean for this dependent variable was .43.

TABLE A.3

REGRESSION OF POST-BASELINE EMPLOYMENT EXPERIENCE[a] AT 12-MONTH INTERVIEW WITH FOUR DIFFERENT TIME-DEPENDENT MEASURES

Explanatory Variable[b,c]	With ENROLDAY	With TGROUP	With SAMPLE	With T2-T7
Age	-.00	-.00	-.00	-.00
White	-.01	-.01	-.02	-.01
Hispanic	-.01	-.01	-.01	-.01
Age of Youngest Child	.00	.00	.00	.00
Mother Present at Baseline	.08+	.08+	.08+	.07+
Mother's Education	.00	-.00	-.00	-.00
Father's Education	.09*	.09*	.08+	.09*
Married at Baseline	.11	.11	.11	.11
Pregnant at Baseline	-.09+	-.09+	-.09+	-.09*
In School at Baseline	.17***	.18***	.18***	.18***
Highest Grade Completed	.04*	.04+	.04+	.04*
Number of Semesters Dropped	.02	.02	.02	.02
Time out of School, Dropouts	-.06	-.06	-.06	-.06
Enrolled in Teen Parent Program	-.07	-.07	-.07	-.07
Employed Pre-Baseline	.16***	.16***	.16***	.16***
ENROLDAY	-.00**	—	—	—
ENROLDAY * GROUP	.01*	—	—	—
TGROUP1	—	.28*	—	—
TGROUP2	—	.09	—	—
TGROUP1 * GROUP	—	-.14+	—	—
TGROUP2 * GROUP	—	-.04	—	—
SAMPLE[d]	—	—	—	—
SAMPLE * GROUP	—	—	.11	—
T2	—	—	—	-.16
T3	—	—	—	-.43+
T4	—	—	—	-.38
T5	—	—	—	-.42
T6	—	—	—	-.39
T7	—	—	—	-.61+
T2 * GROUP	—	—	—	.06
T3 * GROUP	—	—	—	.21
T4 * GROUP	—	—	—	.18
T5 * GROUP	—	—	—	.19
T6 * GROUP	—	—	—	.13
T7 * GROUP	—	—	—	.29
Interval, Baseline to Follow-up	.00	.00	.00	.00
Participated in Project Redirection	.41**	.06	.29**	.31*
Adjusted R^2	.113	.105	.105	.100
Number of Respondents	722	722	722	722

SOURCE: Tabulations are from AIR baseline and follow-up interviews with the aggregated impact analysis sample.

NOTES: The coefficients in this table are unstandardized (b's).

[a] Teens who had any employment experience between baseline and the 12-month interview were coded 1; others were coded 0.

[b] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c] Unless otherwise specified, all explanatory variables are baseline characteristics.

[d] Sample I was coded 0; Sample II was coded 1.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

also included for the interaction between the time measures and the treatment. As Table A.3 shows, the value of R^2 was highest when the continuous variable was used. The Sample I/Sample II dichotomy added no more explained variance than the arbitrary division of the sample into three groups of equal size.

On the basis of these analyses, it was concluded that the aggregation of Sample I and II was justifiable. With the use of reconstructed baseline variables, there appeared to be no important obstacle to pooling the data, assuming that time factors were included in the analyses of employment outcomes.[3]

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[3] Time factors were found not to be important in explaining employment outcomes at 24-months post-baseline, but were important for 12-months post-baseline.

APPENDIX B

SUPPLEMENT TO CHAPTER II: ANALYTIC STRATEGIES

BY

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APPENDIX B

SUPPLEMENT TO CHAPTER II: ANALYTIC STRATEGIES

This appendix augments the discussion presented in Chapter II on the analytic methods used to assess the impacts of Project Redirection. Three analytic issues are discussed: the use of linear versus nonlinear models to estimate program impact; the use of alternative measures of program participation; and special procedures for handling selectivity bias.

A. Linear and Nonlinear Models

Multivariate statistical procedures are generally used to analyze data from quasi-experimental designs in which the potential non-equivalence of experimental and comparison groups prior to the intervention is a major concern. Because the results produced by multivariate analyses may be affected by the particular statistical method employed, the determination of the most appropriate estimation technique for the purpose at hand is crucial to the validity of the results. The statistical problem is to design an appropriate model that is capable of producing unbiased, efficient estimates of the effects of the program or of its various components.

The most widely used analytical technique in quasi-experimental designs such as the one used in this study is analysis of covariance (ANCOVA). ANCOVA is typically employed to adjust estimates of the treatment effect for known differences in the characteristics of subjects. The general ANCOVA regression model for the analysis can be stated:

$$(1) \quad Y_{ijt} = F\,(Z_m, X_{ni}, U_{ijt})$$

where

Y = a vector of individual outcomes (i.e., completion of schooling, scores on a Birth Control Knowledge Test, etc.)

Z = a vector of dimensions or components of the program (e.g., participation versus nonparticipation in the program or the provision versus non-provision of educational counseling services)

X = a vector of personal characteristics (e.g., age, pregnancy history, school status at baseline, etc.)

U = a vector corresponding to a stochastic disturbance or residual term

The subscripts denote that there are i individuals in the sample under consideration; j outcome measures in which we are interested; m dimensions of the program; n personal characteristics; and t the time periods (e.g., number of months) that have elapsed since the individual entered the program.

In this model, outcomes are posited to be a function of two major sets of variables: (1) predetermined factors or covariates; and (2) the effects of participating in the program. Covariates serve two important functions. First, they reduce error variance by attributing a portion of the variation in the dependent variable to exogenous factors. This decreases the standard error of the estimate, producing more efficient (or loosely speaking, precise) estimates of the treatment effects. Second, to the extent that selection differences are associated with specific exogenous variables, covariates will also reduce and possibly eliminate selection biases present in the analysis.

A linear (ANCOVA) model is generally a useful first approximation at estimating program impacts. It is a relatively inexpensive procedure and permits experimentation in developing the best specification of a given relation. However, if the assumptions upon which the ANCOVA model is based are likely to be violated, the resulting estimates may be biased, inconsistent, and/or inefficient. In that case, either the necessary corrections in the model must be made, or an alternative estimation technique to produce reliable estimates must be found and applied. In particular, maximum likelihood procedures are often substituted for ANCOVA to deal with problems of nonlinearity. Nonlinear maximum likelihood estimation techniques, such as logit or probit methods, are often appropriate, for example, when the measure of program outcome takes the form of a binary (dichotomous) variable.

The attraction of these nonlinear estimation techniques lies in the fact that they avoid two major statistical problems that may arise in the use of the linear regression ANCOVA model. When the dependent variable is binary in nature, the error term will be heteroscedastic, resulting in unbiased but generally inefficient estimates of the parameters of the model, and in biased standard errors. An even more serious problem, however, is that the resulting coefficients may imply probability estimates for the dependent variable outside the 0 to 1 range. Since probabilities are not defined outside that range, it is difficult to know how to interpret predictions that do not lie within its bounds. Trying to eliminate the problem by defining predictions of less than zero as equal to zero (or of greater than one as equal to one) is not a uniformly acceptable strategy; in some instances, it can produce an unreasonable clumping of predictions at either bound.

However, logit and probit methods are not without problems of their own. In addition to cost considerations, one rather unattractive feature has to do with the interpretation of the coefficients. While coefficients obtained from the ANCOVA model have a simple interpretation (i.e., they indicate the effect on the dependent variable of a one-unit change in an independent variable holding other predictor variables constant), the interpretation of coefficients arising from the nonlinear forms are less straightforward. In particular, estimates of marginal probabilities obtained from such coefficients are dependent upon the mean values of all other covariates in the equation. While the linear regression coefficients provide an estimate of treatment effects that may apply to other samples, coefficients obtained from nonlinear estimation techniques cannot be directly interpreted in the same manner.

No estimation technique is, therefore, ideal. The proper choice of a method must take into account the specific objectives of a study and the constraints under which it operates. Information furnished in a recent paper by Amemiya (1981) is quite useful in making a judgment in this matter. He demonstrated that, in most instances, logit and probit methods produce equivalent results to ordinary least squares (OLS) regression. Pronounced differences between the methods appear only when the mean of the dependent variable lies near a boundary point (i.e., 0 or 1). More interestingly, he also shows that as long as the mean of the dependent variable lies within the .30 to .70 range, there is likely to be a clear and simple relationship between the coefficients (and hence resulting predictions) produced via the use of logit, probit, and the OLS regression models.

Based upon all these considerations, the following research strategy was adopted. When the dependent variable was binary and when its mean fell within the .30 to .70 range, the linear regression model was generally used, in an attempt to pin down the best specification of a particular relationship. Nonlinear estimations (logistic regressions) were relied upon in presenting the final estimates, but were also used more heavily in the preliminary runs when the mean of the binary dependent variable lay outside the .30 to .70 range.[1]

Generally, then, the results presented in Appendix E were generated by ordinary least squares and logistic regression analysis. There was one outcome, however, for which these methods were not appropriate. For the length of time between the index and a subsequent pregnancy (Table E.8), the analysis had to allow for the fact that some girls did not have a repeat pregnancy over the course of the study and consequently had their time truncated. Since the girls in the experimental group tended to have longer at-risk periods, OLS estimates would have been biased. To compensate for this difference in truncation times, Tobit analysis was used. The truncation point for each observation was the time from the index pregnancy until the last observation.

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[1] Multiple classification analysis (MCA) was, however, used in producing adjusted means and percentages shown in the body of the text. For outcomes whose mean values were outside the .30 to .70 range, results of the MCA were double checked by translating the coefficients from the logit analyses into adjusted percentages.

B. Measurement of Treatment Effects

The effectiveness of Project Redirection could be measured in various ways. The simplest method is to use a dummy variable to indicate whether an individual was a member of the experimental or comparison group. When coded in this manner, the resulting regression coefficient on this binary variable is an estimate of treatment effect; it represents the average difference[2] in the particular outcome measure for program participants relative to comparison group members after adjusting for individual differences.

While the simple dummy variable method offers a straightforward interpretation of the average program effect on an outcome measure, it does not take into account how much or which inputs of a program individual participants had been exposed to. It is plausible to expect individuals who were exposed only briefly to a program to benefit less than individuals who were exposed to that program longer. Therefore, the amount of time individuals spent in a program could be substituted for the simple dichotomous participation variable in the ANCOVA specification. The resulting coefficient on this time-in-program variable would then correspond to the change in outcome measure associated with an incremental unit of time involvement.

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[2] In the case of logit analysis, it is a transformation of the average difference.

There is, however, no strong *a priori* reason to impose the assumption that the effectiveness of a program is linear with respect to the amount of time spent in it. In particular, individuals who were enrolled for only a short period may have spent most of that time becoming oriented to the program. Later months might have been more productive in terms of impact on individual outcomes. But it might also be true that the marginal effectiveness of additional months spent in the program declined after some point--that is, after the participants were properly exposed to its most beneficial aspects. To test whether nonlinearities of this sort are present, both a linear (time enrolled) and second degree term (time enrolled squared) may be used on the right-hand side of the specification to measure program effect. Together, these terms are capable of approximating most kinds of nonlinearities that are likely to exist with respect to program effectiveness.

While the methods described above can provide an indication of program effectiveness, they cannot specify which features of a program work especially well and which do not. Certain features may be assessed, however, by employing additional dummy variables on the right-hand side of the specification to reflect whether subjects received particular services through the program or other agencies. For an examination of employment outcomes, for instance, one alternative is to add two additional dummy variables to the model to indicate whether a subject received employment training as part of the program or received similar services from some other agency.

While the logic of using alternatives to the dummy variable indicator of program participation is straightforward, the interpretation is not. What one would like to learn is whether increased program exposure, or exposure to different program components, results in improved outcomes. The problem is that these treatment variables are confounded with subject characteristics. When length of enrollment, for example, is used as the treatment variable, we are measuring both intensity of program inputs and client characteristics (such as perseverance or motivation) that lead teens to take advantage of the programs services. In other words, teens select themselves into different periods of program stay and into receipt of specific services. If length of stay is found to have a positive impact on outcomes, there is no way of knowing definitively if teens who remained a long time would have done as well even with shorter enrollments because they were more motivated, more aggressive, more competent, and so on.

Despite these problems, all three approaches outlined above were used to assess the impacts of Project Redirection. However, it is important to recognize the interpretive complexities when treatment is not measured as a dichotomous (experimental/comparison) variable. While the inclusion of covariates known to be associated with length of stay in the program should reduce the self-selection problem, it will not eliminate this bias. Therefore, caution should be exercised in coming to conclusions about the effects of service intensity or receipt of a specific service on outcomes of interest.

Regression tables showing the results of the impact analyses are presented in Appendix E. Tables for nonlinear effects of length of program enrollment are not included because in no case did the second degree term (time enrolled squared) prove to be statistically significant. Also not included are the full regression tables for 12-month impacts. These tables were included in the interim report on program impacts (Polit et al., 1983).

C. Selectivity Bias

Chapter 2 described several design strategies that were introduced to minimize the threat of selection bias. Despite the research design, baseline differences were observed between the experimental and comparison groups, as shown in Table 2.1.

There are several possible approaches for dealing with this problem analytically. The most common is to use multivariate procedures such as ANCOVA to control baseline characteristics. A potential shortcoming of this approach, however, is that the available covariates may not control for all relevant group differences contributing to (either program or self) selection bias. If certain relevant factors, such as entry-level aspirations, ability, or motivation, are not measured or included as covariates, the regression specification will result in only a partial adjustment for differences between groups. Remaining differences will be "forced" into the residual term, very likely violating the hypothesized characteristics of the distribution of that variable. Biased estimates will then be produced if the residual is correlated with program treatment, as would be the case if atypically motivated or atypically disadvantaged individuals were participating in the Redirection program.

There are several possible approaches for dealing with this problem. One is to develop and use some proxy to represent the aspiration, motivation, or capability factors. A likely candidate is the baseline (pre-program) measure of the outcome under consideration. We would expect, for example, that girls employed at baseline would be more likely than other teens to be employed at follow-up, regardless of whether they were involved in Redirection or not; having a job at baseline is probably an indication of both the teen's employability and her motivation to seek out a job. This would likely be true even if typical job duration were short (i.e., even if the particular job at baseline did not last into the follow-up period). Standardizing for baseline employment represents an attempt to control for pre-program employability and motivation.

This is a relatively simple way of dealing with the selection problem, but it may not always represent the missing variables satisfactorily. Baseline measures may not conform purely to the "permanent" characteristic of interest, and may also be affected by "transitory" factors. For example, girls who were mothers at baseline might have dropped out of school to care for their young children. But among these young mothers, the desire or motivation to return to school at that point might have differed systematically between program participants and non-participants. If so, then controlling for school enrollment at baseline would not necessarily standardize effectively for the school motivation factor.

An alternative, but not mutually exclusive, approach to ANCOVA attempts to capture motivational, attitudinal, or ability differences among individuals indirectly by moving beyond the single-equation regression specification. This alternative was recently developed by economists for investigating many aspects of individual behavior. It is a sophisticated statistical methodology involving an adjustment for selection bias by first modeling the selection process that segregates subjects into the treatment and comparison groups.

According to this approach, if unobserved variables, such as motivation or ability, affect both the outcomes of interest and the decision to participate in a program, then group status is potentially endogenous with behavioral outcomes. Since single-equation estimators will generally be biased and inconsistent in this case, a two-stage estimation procedure is necessary. First, the selection process is modeled by performing a maximum likelihood logit or probit analysis of the relationship between the group status dummy variable and factors hypothesized to influence program participation. In the second stage, the first stage results are used to develop a correction factor (the inverse of Mill's ratio) that is then inserted into the model's second equation (Heckman, 1979).

Although this procedure is attractive because it allows us to correct for the omission or improper measurement of certain variables that may be important in determining individual outcomes, it is not without its difficulties. For the procedure to be useful, it is necessary that the first-stage equation describe (i.e., predict) the selection process reasonably well. This is not always easy to accomplish. If it cannot be achieved, resulting estimates of program effect that appear in the second stage are likely to be sensitive to the information incorporated from the first stage; i.e., the estimates are not "robust." If so, we can place little confidence in the second-stage estimates of program effect. In fact, this is precisely the situation that developed when the Heckman procedure was applied in an attempt to correct self-selection biases in the present study.

The first-stage model predicting program participation included the following baseline predictors: marital status, ethnicity, school status, number of pregnancies, employment status, household income, mother's education, enrollment in a teen parent program, presence of mother in the household, and work attitudes. This model, as well as others that were developed, were not very successful in predicting program participation. Either because of the homogeneity of the population, the design used to match groups, or the failure to measure key variables, it proved to be very difficult to develop a prediction equation for experimental versus comparison group status. In fact, the highest amount of explained variance was .13, corresponding to a relatively poor fit and substantial errors of estimate. A second problem was that participation estimates were primarily dependent on variables that were also used as covariates to predict outcomes, resulting in substantial redundancy. The net result was that the correction factors were not statistically significant, and their inclusion had no effect on the coefficients for the participation variable (i.e. statistically significant results remained significant and non-significant results remained non-significant). Our conclusion was that the use of ANCOVA accomplished as much as could be done, given the research design, to correct self-selection biases.

The Heckman procedure was also applied to correct attrition biases resulting from sample losses over the 24-month study period. The results are similar to those described above and are explained in greater detail in Appendix C.

D. Life Table Analysis

As indicated in Chapter 4, the analysis of fertility-related outcomes had to adjust for the fact that teens entered the study at different points in their pregnancy or parenting experiences. Life table analysis was one method used to deal with this issue.

Life table analysis is a statistical procedure that evaluates the time interval between two events: a starting event (here, termination of the index pregnancy) and a terminal event (here, the onset of a subsequent pregnancy.[3] In life table analysis, this interval (or survival time) is measured from an individual's own starting event, not from the time the study began. This feature is useful in the present study since subjects entered at different times, became pregnant at different times relative to entry, and were followed up for various intervals relative to their index pregnancies. Life table analysis has been used in many studies of contraceptive use and failure (e.g. Potter, 1966, Jain and Sivin, 1977; Tietze and Lewit, 1973; Chandrasekaran and Hermobin, 1975), as well as in several studies of teen pregnancy (e.g. Furstenberg, 1976; Currie et al., 1972; Koenig and Zelnik, 1982; and Testa, 1983). While in many respects the technique is well suited to the characteristics of the research design in the present study, its major shortcoming is its inability to statistically adjust for background differences between groups. Given the potential selection biases discussed in Chapter 2, this shortcoming is an important one. However, life table analysis has been included because of its widespread use in other similar studies.

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[3] In life table analysis, if the terminal event has not occurred by the end of the study, the survival time is the interval between the starting date and the time the study ended. Such cases are referred to as censored cases because the time to a subsequent pregnancy (if any) is not known exactly, but is known to be of at least a certain duration. The life table technique includes both censored and uncensored cases in developing a sumulative survival rate (proportion of cases surviving to the end of a specified time period) for groups of subjects.

Since background variables could not be statistically controlled in these life table analyses, separate analyses were performed for teens who were in or not in school at baseline, the characteristic that most distingiushed the experimental and comparison teens. Figure B.1A shows the cumulative percentage of teens who "survived" (i.e. avoided another pregnancy) among teens who were not in school at baseline, for specified intervals after the index pregnancy was terminated. For this subgroup, the experimental teens had lower percentages of subsequent pregnancies (i.e., higher survival rates) than comparison teens at any given point postpartum, but the differences were small. For teens in school at baseline (Figure B.1B), the two groups were quite similar until after 24 months postpartum, when rates of subsequent pregnancy for the comparison teens rose sharply, while that for experimental teens leveled off (the number of cases in the tail, however, was small). In both instances, the difference in the mean survival scores[4] for the two groups was not statistically significant. Separate survival analyses were performed for various subgroups (blacks, Puerto Ricans, Mexican Americans, younger teens, older teens, teens pregnant at baseline, and teen mothers at baseline). Experimental teens generally had somewhat better survival curves than comparison teens, but differences were small and not statistically significant.

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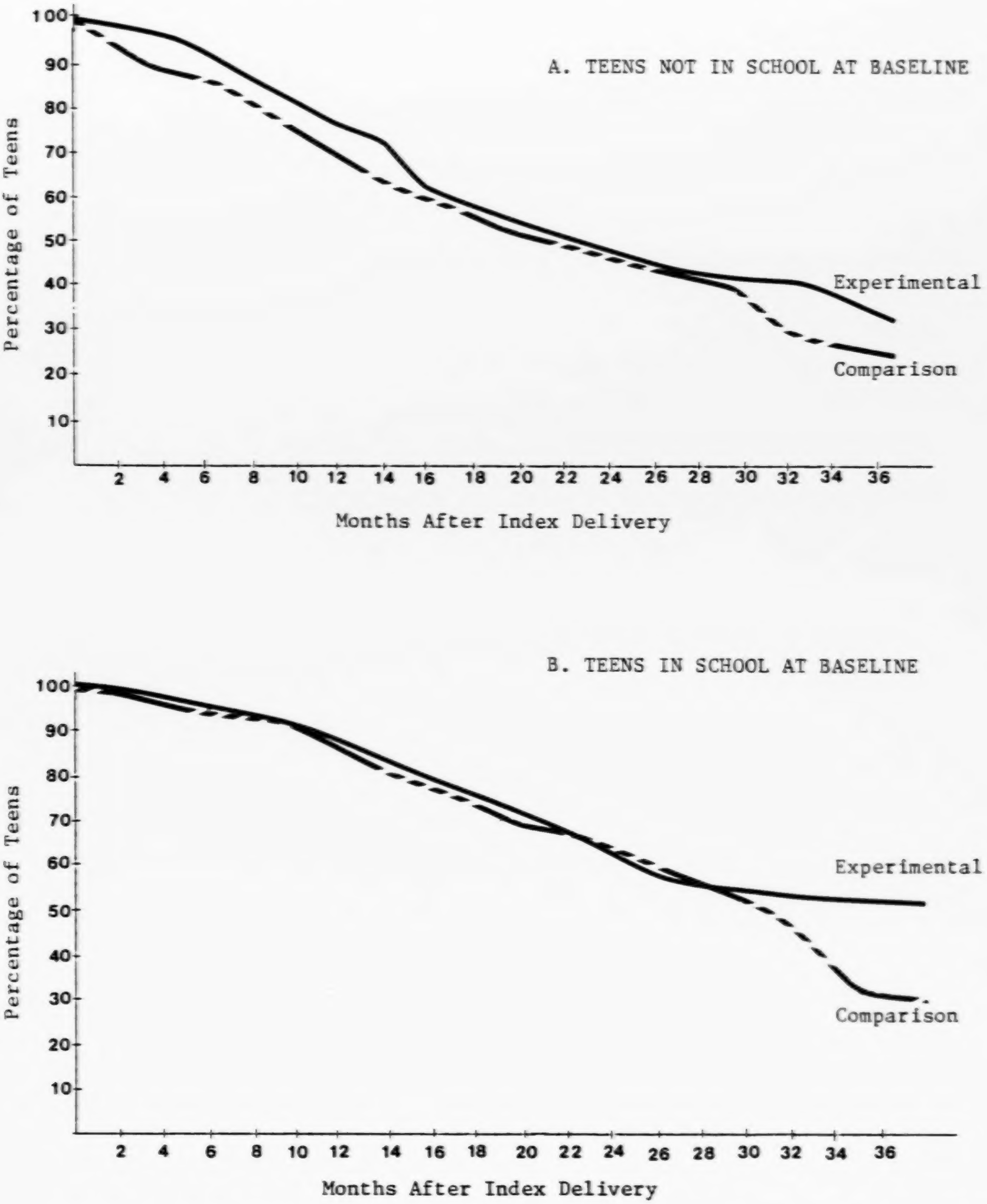
[4] In survival analysis, the survival distribution of two groups can be compared by computing a D statistic, which is based on the computation of a mean survival score for the two groups. For each individual a score U is computed by comparing a subject's survival time with that of all other subjects. This score is initially zero and is incremented by one for each case whose survival time is less than the subject's and decremented by one for each case whose survival time is greater than that of the subject (Hull and Nie, 1979).

FIGURE B.1

CUMULATIVE PERCENTAGES OF TEENS WITHOUT A SUBSEQUENT PREGNANCY AT SPECIFIED INTERVALS AFTER INDEX DELIVERY, BY GROUP



SOURCE: Tabulations are from AIR interviews with experimental and comparison group members at baseline, 12 months post-baseline, and 24 months post-baseline.

NOTES: Index delivery refers to the delivery of the pregnancy in progress at baseline or the most recent delivery prior to baseline.

APPENDIX C

ATTRITION IN THE IMPACT ANALYSIS SAMPLE

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Overall, the attrition of subjects from this study was reasonably low. As indicated in Chapter 2, the completion rate for Sample I was 86% between baseline and the final interview. For Sample II teens, 85% of those initially interviewed (at 12 months post-baseline) were interviewed again at 24 months post-baseline. Given the characteristics of the sample (their youth, recent pregnancy, poverty, and their residence in primarily urban areas where a distrust of strangers is common), a 15 percent attrition rate after two years can be considered low.

Nevertheless, there are reasons for concern about the possibility of selective attrition. A loss of 15% of the sample is not large but could nevertheless result in attrition biases. The major problem, however, is that there was significantly more attrition in the experimental group than in the comparison group. In Sample I, only 79% of the Redirection teens were re-interviewed 24 months post-baseline, compared with 93% of the comparison teens. A similar group difference was observed in Sample II. Some possible explanations for this difference were discussed in Chapter 2. These explanations center primarily on the differential handling of the two groups by the survey team and do not necessarily implicate motivation, degree of disadvantage, or other subject characteristics that could bias the findings. However, such biases could not be ruled out.

Attrition biases, like selection biases, could run in either direction. On the one hand, if teens who were highly disadvantaged were more likely to drop out of the experimental group than other teens, a positive selection bias could arise in the follow-up data (if these were the teens "beyond the help" of program services). On the other hand, if attrition in the experimental group favored those who initially were least disadvantaged (e.g. those who were able to find employment and establish an independent household), then a negative attrition bias could arise.

To explore the nature and direction of attrition biases, if any, teens who completed the final interview were compared with teens who did not in terms of characteristics measured in the initial interview. The results are summarized in Table C.1 for Sample I and C.2 for Sample II.

Table C.1 suggests that completers were not a random subgroup of all Sample I teens. Completers and non-completers were significantly different on four of 21 characteristics: percent living with their mothers, mean highest grade completed, percent in a teen parent program at baseline, and percent pregnant at baseline. However, several further observations about this table are in order. First, the two groups were not significantly different for the majority of characteristics examined, including many not shown in the selected list in Table C.1. Second, the teens were comparable with respect to most of the characteristics that were found to be the most powerful determinants of follow-up outcomes (e.g. ethnicity, age, baseline school status, family size, baseline employment record, baseline welfare status, and number of baseline

TABLE C.1

COMPARISON OF SAMPLE I TEENS WHO COMPLETED OR DID NOT COMPLETE A 24-MONTH INTERVIEW ON BASELINE CHARACTERISTICS

Baseline Characteristic	Teens Who Completed 24-Month Interview	Teens Who Did Not Complete 24-Month Interview	Difference
Mean Age	15.9	15.9	0.0
Percent Married	6.2	6.3	- 0.1
Percent Living With Mother	69.4	50.8	18.6**
Percent Whose Mother Was a Teenage Mother	71.5	61.9	9.6
Percent Having Lived in Same Location Previous 12 Months	36.0	34.9	1.1
Mean Number of Siblings	5.5	5.1	0.4
Percent Black	45.3	52.4	- 7.1
Percent Hispanic	42.7	38.1	4.6
Percent White	10.6	7.9	2.7
Percent in School	56.5	55.6	0.9
Mean Highest Grade Completed	8.6	8.2	0.4*
Percent in a Teen Parent Program	26.8	44.4	-17.7**
Percent of Dropouts Planning to Return	81.8	91.7	- 9.9
Percent Wanting More Than a High School Diploma	39.1	47.5	8.4
Percent Employed	7.0	6.3	0.7
Mean Number of Jobs Held	1.2	1.3	- 0.1
Percent Who Would Rather Work Than be on Welfare	91.5	92.1	- 0.6
Percent in an AFDC Household	20.8	12.7	8.1
Percent Pregnant, not a Parent	56.0	69.8	13.8+
Percent With More Than One Pregnancy	20.8	12.7	8.1
Mean Number of Services Used, Past Three Months	3.8	4.2	0.4
Number of Respondents	386	63	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I only at baseline.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.

TABLE C.2

COMPARISON OF SAMPLE II TEENS WHO COMPLETED OR DID NOT COMPLETE A 24-MONTH INTERVIEW ON BASELINE AND 12-MONTH CHARACTERISTICS

Characteristic[a]	Teens Who Completed 24-Month Interview	Teens Who Did Not Complete 24-Month Interview	Difference
Mean Age	16.5	16.7	- 0.2
Percent Married	6.6	8.0	- 1.4
Percent Raised by Both Parents	20.9	10.0	10.9+
Percent Living With Mother at 12 Months	59.6	46.0	13.6+
Percent Whose Mother Was a Teenage Mother	74.9	74.0	0.9
Mean Number of Siblings	4.7	4.4	0.3
Percent Black	47.0	42.0	5.0
Percent Hispanic	43.9	46.0	2.1
Percent White	6.6	10.0	3.4
Percent in School at Baseline	54.7	44.0	13.7
Percent in School at 12 Months	58.9	62.0	- 3.1
Mean Highest Grade Completed	8.8	8.8	0.0
Percent of Those in School in a Teen Parent Program	28.6	31.8	3.2
Percent Employed at Baseline	9.8	4.0	5.8
Percent Employed at 12 Months	8.7	6.0	2.7
Mean Number of Jobs Held	0.8	0.6	0.2
Percent in an AFDC Household	53.9	66.0	12.1
Percent Pregnant, not a Parent	65.5	72.0	- 6.5
Percent With More Than One Pregnancy	15.0	16.0	- 1.0
Mean Number of Services Used Baseline to 12 Month Interview	4.6	4.6	0.0
Number of Respondents	287	50	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample II only at 12-month interview.

NOTES: [a]Unless otherwise stated, the characteristic specified is a baseline variable.

+Statistically significant at the .10 level.

pregnancies). Third, at least one of the significant differences was for a variable that can be explained in terms of tracking efforts and may not have any relation to motivation, ability and so forth: teens who were living at home at baseline were more likely to be re-interviewed than teens who were not. Finally, the remaining three significant differences do not suggest a compelling trend toward biases in one direction or the other.

Similar observations apply to the data presented in Table C.2 for Sample II teens. For Sample II teens, only two significant differences emerged: percent living with their mothers and percent raised by both parents. Both variables could reflect primarily tracking logistics, although it certainly seems reasonable to conclude that teens from less stable home backgrounds were somewhat more likely to exit the study. In general, though, completers and non-completers in Sample II looked remarkably similar in terms of characteristics measured in the first interview.

Although these comparisons do not suggest a major bias in either direction resulting from attrition, further analyses were performed to test whether corrections for selective attrition would result in changes in program impacts. These analyses involved the two-stage Heckman approach described in Appendix B. In the first stage, probit analyses was used to model attrition, which yielded a correction factor for the second-stage regressions.

Before presenting the results of these analyses, two difficulties should be mentioned. First, in performing these analyses, the two samples were combined, resulting in the loss of a few baseline variables from Sample I that might have been used in the first step of the procedure to predict attrition. Second, the single best predictor of attrition was the participation variable itself, followed by site dummies that were correlated with group status. Given the results shown in Tables C.1 and C.2, it is not surprising that modeling attrition was extremely difficult. The amount of explained variance for the prediction equation was .03 without the participation and site dummies and .10 with them.[1] Thus, the correction factor had a high standard error, making it unlikely to have an effect of much magnitude in the second step.

The results of the second step of the Heckman procedure are presented in Table C.3 for an outcome variable for which a significant program effect was observed, enrollment in school at any time post baseline. The left panel of this table shows probit coefficients uncorrected for attrition. The right panel shows coefficients after inclusion of the correction factor. The results are virtually unchanged. The effect of program participation is highly significant both before and after the Heckman procedure is applied.

The Heckman procedure was used with several other outcome measures. Table C.4 presents the results for an outcome for which the uncorrected OLS regression revealed no group difference, number of pregnancies at follow-up. Because OLS was used, the correction factor (called Lambda) is included as a variable in the right-hand panel. This correction factor was not statistically significant. Furthermore, it had little effect on the participation variable, which continued to be nonsignificant. Including lambda resulted in only minor changes to the coefficients and

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[1] Besides the group and site dummies, the only variable that was a significant predictor of attrition in the first step was pregnancy status at baseline.

TABLE C.3

PROBIT ANALYSIS OF TEENS EVER ENROLLED IN SCHOOL POST-BASELINE, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION, WITH AND WITHOUT CORRECTION FACTOR[a]

Explanatory Variable[b,c]	Without Correction		With Correction	
Age at First Birth	.01	(.03)[d]	.02	(.02)
Black	- .07	(.24)	- .07	(.22)
Puerto Rican	- .59*	(.24)	- .59*	(.26)
Mexican American	- .22	(.24)	- .22	(.25)
Number of Siblings	- .04+	(.02)	- .04	(.02)
Married	- .47*	(.23)	- .47*	(.24)
In School or GED Program	1.25***	(.17)	1.25***	(.18)
Number of Times Dropped Out of School	- .16	(.10)	- .16	(.10)
Enrolled in a Teen Parent Program	.19	(.22)	.19	(.21)
Highest Grade Completed	.05	(.06)	.05	(.06)
Number of Baseline Pregnancies	- .21	(.14)	- .21	(.15)
Number of Jobs Ever Held	.21**	(.07)	.21**	(.08)
Participated in Project Redirection	.64***	(.14)	.64***	(.19)
Constant	.19		.19	
Number of Respondents	662		662	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] The correction factor is based on the procedure developed by Heckman (1979) in which characteristics affecting sample attrition are taken into account.

[b] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c] Unless otherwise specified, all explanatiory variables are baseline characteristics.

[d] The coefficients shown are standardized (Betas). The numbers in parentheses are the standard errors of the Betas. The coefficients, when multiplied by .23, will yield estimates of OLS coefficients.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE C.4

ORDINARY LEAST SQUARES REGRESSION OF NUMBER OF PREGNANCIES AT 24-MONTH INTERVIEW, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION, WITH AND WITHOUT CORRECTION FACTOR[a]

Explanatory Variable[b,c]	Without Correction		With Correction	
Age at First Birth	- .02+	(.01)[d]	- .02	(.02)
Black	.07	(.06)	.07	(.09)
Puerto Rican	.04	(.06)	.03	(.09)
Mexican American	.06	(.07)	.07	(.10)
Number of Siblings	.01	(.01)	.01	(.01)
Married	.08	(.08)	.07	(.08)
In School or GED Program	.04	(.05)	.05	(.06)
Highest Grade Completed	.01	(.02)	.01	(.03)
Number of Times Dropped Out of School	.13***	(.03)	.13*	(.04)
Number of Semesters in a Teen Parent Program	.05*	(.02)	.05*	(.02)
Pregnant at Baseline	- .49***	(.04)	- .49***	(.06)
Number of Baseline Pregnancies	.82***	(.04)	.83***	(.05)
Ever Used Oral Contraceptives	.07	(.04)	.07	(.04)
Number of Days at Risk to Post-Index Pregnancy	- .00***	(.00)	- .00***	(.00)
Number of Months, Baseline to 24-Month Interview	.05***	(.01)	.05	(.03)
Participated in Project Redirection	.02	(.04)	- .01	(.06)
Lambda (Correction Factor)	—		.06	(.20)
Constant	.61		.62	
Adjusted R^2	.62		.62	
Number of Respondents	661		661	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] The correction factor is based on the procedure developed by Heckman (1979) in which characteristics affecting sample attrition are taken into account.

[b] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c] Unless otherwise specified, all explanatiory variables are baseline characteristics.

[d] The coefficients shown are unstandardized (b's). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

the standard errors for other predictor variables. Overall, this analysis yielded little evidence that the correction for attrition bias was affecting program impacts.[2]

This conclusion was supported in analyses with several other outcome variables. In general, when the participation variable was significant prior to the correction, it remained significant after the correction; when it was nonsignificant initially, it remained nonsignificant.

In summary, the participation variable was the best predictor of sample attrition in this study. After controlling for group status, few other variables were predictive of whether a teen would complete the final interview or be lost in follow-up. The amount of variance that the prediction model explained was only 10%. In one sense, this is an encouraging finding. The interviews measured numerous characteristics that could have been associated with attrition: family background, baseline school status, welfare status, family size, number of pregnancies, employment history, highest grade completed, and so on. To the extent that attrition bias was present, it was either small or unmeasured by the variables in the interviews.

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[2] Because Samples I and II were combined for these analyses, certain baseline variables that could have been used as predictors in the first-stage model for Sample I had to be eliminated (e.g. mother present in the household at baseline). To test whether the results would be different if a better first-stage model were used, the Heckman procedure was applied to Sample I cases alone. In fact, a somewhat better first-stage specification was realized. The amount of explained variance in modeling attrition increased to 16%. However, it is clear that factors contributing to attrition were largely unmeasured in either sample. The second-stage results for the Sample I cases consistently showed that the correction factor had no effect on program impacts.

In any event, the low predictive power of the first-stage model of the Heckman procedure resulted in virtually unchanged second-step results. Consequently, all of the analyses presented in the main body of this report and in Appendix E are uncorrected for attrition. We conclude on the basis of these supplementary analyses that the differential loss of experimental and comparison subjects probably had only a modest effect on the outcomes reported in this document and that the direction of these modest potential biases is unclear.

APPENDIX D

REVIEW OF OUTCOMES REPORTED IN OTHER STUDIES OF TEEN PARENTS

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REVIEW OF OUTCOMES REPORTED IN OTHER STUDIES OF TEEN PARENTS

Because the comparison group strategy was not completely satisfactory, it seemed desirable to develop additional perspectives on program effects by examining data from other studies on teen parents. To this end, numerous evaluation reports and studies based on national surveys of similar populations were reviewed. While many of these sources did provide information relating to outcomes examined in this study, there are nevertheless numerous problems that make direct comparisons difficult. In the evaluation reports, methodological weaknesses generally undermined the credibility of the results. A particularly serious problem was the failure of most evaluations to correct for or even assess attrition and self-selection biases. Data were generally obtained only from teens whom the programs were still serving or with whom they were still in contact. Most evaluations were also done with small samples, which could affect the stability of their results.

With respect to both the evaluations and national surveys, an additional problem is that the teens in the present study were generally more disadvantaged and younger than teens in other samples. Consequently, it is possible and perhaps even likely that teens in Project Redirection would not compare favorably with teens in these other studies. Nevertheless, because of an interest in placing the results of this study into broader perspective, some comparative data are presented below. In order to interpret the comparisons, it is important to keep the characteristics of the data sets in mind. Table D.1 summarizes the major sources of comparative data and highlights differences between the samples from which they are drawn and the Project Redirection impact analysis sample. Data sets with fewer than 100 subjects or data collected prior to 1970 are generally omitted from this discussion.

Repeat Pregnancies

The first outcome for which a comparison was made is the rate of repeat pregnancies. Table D.2 summarizes the data from six of the more reliable sources. As this table indicates, rates of subsequent pregnancy are fairly comparable in all data sets at 12 months after an earlier pregnancy is terminated. By 18 and, especially, 24 months postpartum the various data sets provide different estimates of repeat pregnancy rates. The teens in the Redirection sample--both experimental and comparison subjects--had higher rates of repeat pregnancies 24 months postpartum than any other sample except that of Klerman and Jekel. The sample best matched to the Redirection sample demographically--the Illinois AFDC receipients--had lower rates for every ethnic group. In this cross-sectional study, it is possible that underreporting of pregnancies was higher than in the present one, but this is purely speculative.

Omitted from Table D.2 are three additional sources of information. The first is from the evaluation of OAPP-funded projects (Burt et al., 1984), which presented information for pregnancy status at 12 and 24

TABLE D.1

SUMMARY OF CHARACTERISTICS OF MAJOR COMPARATIVE DATA SETS

Name and Source	Sample Size	Age	Ethnicity	Social Class	Additional Comments
Project Redirection	675	≤ 17	46% Black 42% Hispanic	70% in an AFDC household; 100% poor	Fairly high levels of service receipt in experimental and comparison groups; 6% were married; about 55% were in school at baseline; completion rate was about 86%.
OAPP-Funded Project Clients (Burt et al., 1984)	3884	≤ 19	49% Black 8% Hispanic	28% on AFDC	The data were collected through the projects' management information systems, not through in-depth personal interviews; attrition rate was about 30% at 12 and 24 month follow-ups; most teens (68%) were in school or graduated at program entry; 11% were married.
Illinois AFDC Recipients (Testa, 1983)	1900	≤ 18	54% Black 13% Hispanic	100% on AFDC	The service receipt patterns of these teens is unknown; presumably only some received special services; this was a cross-sectional survey with a 79% completion rate.
1979 Survey of Young Women in Metropolitan Areas (Koenig and Zelnik, 1982)	320	15-19	59% Black no Hispanics	Mixed	The service receipt patterns of these teens is unknown; 100% were single; completion rates in this cross-sectional survey are unknown but presumed to be about 85%.
1973-1976 National Survey of Family Growth (Ford, 1983)	442	≤ 19	51% Black no Hispanics	48% Below 150% of Poverty	The service receipt patterns of these teens is unknown; completion rates are unknown but presumably high; 49% single at first birth.
1979 National Longitudinal Surveys of Work Experience of Youth (Mott and Maxwell, 1981)	695	≤ 22	37% Black no Hispanics	39% on AFDC	The service receipt patterns of these teens in unknown; completion rates unknown but presumably high; 42% single at time of interview.
John Hopkins Adolescent Pregnancy Program Clients (Hardy et al, 1981)	1562; 200 in follow-up	≤ 18	80% Black no Hispanics	Unknown	Comparison of clients of special and regular hospital-based services; follow-up attrition was about 50%, after two years.
Young Mothers' Program, New Haven, Conn. Clients and Controls (Klerman & Jekel, 1973)	263	≤ 18	90% Black 3% Hispanic	75% on AFDC	Comparison of clients of special hospital-based services with those of an obstetric clinic; 82% completion rate after 25 months for experimentals; low but unspecified completion rate for controls; 100% were single
Sinai Hospital, Baltimore Clients and Controls Prenatal Clinic (Furstenberg, 1976)	544	≤ 18	90% Black no Hispanics	21% on AFDC during childhood	Comparison of clients of special hospital-based services with their classmates; 90% completion rate after three years for experimentals and 74% for classmates; 54% lived with both parents initially; 22% of experimentals had completed high school initially; 19% were married initially; 75% initially in grade for age.

TABLE D.2

COMPARISON OF RATES OF REPEAT PREGNANCY IN VARIOUS DATA SETS, BY SPECIFIED INTERVALS

Name and Source of Data Set	12 Months Postpartum	18 Months Postpartum	24 Months Postpartum
Project Redirection[a]	21% Experimentals 23% Controls	34% Experimentals 38% Controls	47% Experimentals 52% Controls
Illinois AFDC Recipients[b] (Testa, 1983)	21% Blacks 20% Hispanics 13% Whites	34% Blacks 28% Hispanics 25% Whites	42% Blacks 32% Haspanics 28% Whites
1979 Survey of Young Women[b] (Koenig and Zelnik, 1982)	20% Blacks 21% Whites	27% Blacks 31% Whites	37% Blacks 39% Whites
National Survey of Family Growth (Ford, 1983)	17% Black 17% White	---	---
John Hopkins Program (Hardy et al., 1981)	8% Experimentals 21% Controls	--- ---	25% Experimentals 39% Controls
Young Mothers' Program[b] (Klerman and Jekel, 1973 ; Also Currie et al., 1972)	25% Experimentals	38% Experimentals	51% Experimentals 66% Controls
Sinai Hospital Prenatal Clinic[b] (Furstenberg, 1976)	23% Experimentals	---	43% Experimentals

NOTES: [a]The Redirection percentages are adjusted for background characteristics. Most of the rates shown for other data sets are unadjusted.

[b]The percentages shown reflect survival rates obtained from life-table analyses.

months postpartum (i.e., pregnancies beginning and ending in between follow-ups were not included). In the OAPP study, 10% of the teens were actually pregnant at 12 months and 17% were pregnant at 24 months. In Project Redirection, the rates were 10% for experimentals and 13% for the controls at the 12 month interviews and 13% for both groups at the 24 month interviews. Actual rates of pregnancy for the Project Redirection teens at 12 and 24 months postpartum (as opposed to post-baseline) are unknown but presumed to be within 1-2% of these figures. Thus, Project Redirection clients and their comparison counterparts appear to have repeat pregnancy rates similar to that of the clients in OAPP-funded projects.

Also not indicated in Table D.2 are two sources that provide information on the amount of time elapsed between pregnancies. According to birth records from 49 states, 62% of all teens who had a higher order birth between the ages of 15 and 19 delivered within 23 months of an earlier birth in 1981 (U.S. Center for Health Statistics, unpublished data). This means that nearly two-thirds of young teen parents become pregnant within 14 months of an earlier birth (assuming nine months gestation), not including any abortions or miscarriages. In the present sample, among all those with a repeat pregnancy before age 20, 56% of the experimental and 57% of the comparison teens were pregnant again within 14 months after the index pregnancy was terminated. Thus, these national data suggest better rates of repeat pregnancy among both comparison and experimental teens in the present study than for the general population of teen parents.

The Collaborative Prenatal Project of the National Institute of Neurological Disorders, which collected longitudinal data from over 35,000 pregnant women, also provided information on intervals between pregnancies (Broman, 1978). According to this source, the mean interval between a delivery and a subsequent pregnancy for teens age 17 or younger at the time of the higher-order pregnancy was 6.6 months for white teens (N=152) and 9.0 months for black teens (N=596). For comparably-aged teens in the present study, the mean intervals were 12.8 and 13.7 months for experimental and comparison teens, respectively.

In summary, it is not clear from this review of other data whether teens in Project Redirection had better or worse repeat pregnancy rates at the end of the study than one would expect from unserved teens from similar backgrounds. The noncomparability of the samples and differences in the research designs (e.g. cross-sectional versus longitudinal) are major problems in interpreting these conflicting results. Nevertheless, if the Redirection data are accurate, the subsequent pregnancy rates for both groups in this study appear to be lower than that for teen parents in general, based on comparative data from the most accurate source (birth records).

Health Outcomes

Although the Project Redirection impact analysis only collected limited data on health outcomes, it is possible to compare the rates of low birth weight infants for teens in the present study with those from other samples. Table D.3 summarizes the comparative data, drawn from numerous evaluations and national surveys. The table includes several data sets not described in Table D.1. In general, these additional data sets are from small-scale local evaluations of hospital-based programs that serve young, predominantly minority teen mothers. The data in this

TABLE D.3

COMPARISON OF RATES OF LOW BIRTHWEIGHT INFANTS BORN TO TEEN MOTHERS IN VARIOUS DATA SETS

Name and Source of Data Set	Treatment Group	Non-Treatment Group
Project Redirection	7%	7%
National Center for Health Statistics (1982), Birth Certificate Data for 1980	--	14% Nonwhites Under Age 17
OAPP-Funded Projects (Burt et al., 1984)	7%	--
Young Mothers' Program (Klerman and Jekel, 1973)	12%	21%
Illinois AFDC Recipients (Testa, 1983)	--	12%
Johns Hopkins Adolescent Pregnancy Program (Hardy et al., 1981)	14%	13%
Rochester Adolescent Mothers' Program (McAnarney et al., 1978)	8%	11%
Young Mothers' Educational Development Program, Syracuse (Osofsky & Osofsky, 1970)	8%	--
Parent - Infant Interaction Program, St. Louis (Flick, 1983)	6%	15%
Collaborative Perinatal Program (Broman, 1978)	--	15% Blacks Under Age 18 8% Whites Under Age 18
Yale Teen Obstetrical Clinic (Dickens et al., 1973)	8%	15%
Teen Parents Project, Howard University (Washington and Rosser, 1981)	9%	14%
Teen Clinic, Kings County Hospital (Chanis et al., 1979).	11%	--
Teen Clinic, Pennsylvania Hospital (Jorgensen, 1972)	11%	20%
Young Mothers' Clinic, East Meadow, N.Y. (Knapp and Drucker, 1973)	5%	13%
Margaret Hudson Program, Oklahoma (West-Anderson, 1978)	8%	12%

table consistently indicate that girls served by special teen parent programs have lower rates of low birthweight infants than teens either receiving regular medical care or in the general population. The rate for both the experimental and comparison teens in this study who delivered subsequent to baseline compare favorably with that for almost every other group listed in this table.

Educational Outcomes

Comparative data regarding the educational outcomes of teen parents are presented in Table D.4. The percentages of teens who were either attending or who had completed school is shown for specified intervals after the delivery of an index pregnancy for four studies. Unfortunately, none of these provides a very good basis of comparison for Project Redirection. One problem is the different racial mix of the various studies. Since black teen mothers generally have higher rates of school attendance than other teen mothers, the percentages reported by Klerman and Jekel (1973) might be high, while those reported in Burt et al. (1984) might be low relative to teens like those in the Redirection sample. On the other hand, the lower rates of poverty and AFDC receipt and the older ages of the subjects in Burt et al. (1984), as well in Mott and Maxwell (1981), would probably result in overestimated rates of positive schooling relative to the Redirection sample.

Data from the Illinois AFDC sample (Testa, 1983) probably provide the most appropriate comparison in terms of sample characteristics, but are problematic from another perspective. The study reported school attendance rates at the time of the interview, not at any fixed interval after a delivery. The report stated only that 42% of those whose child

TABLE D.4

COMPARISON OF RATES OF SCHOOL ATTENDANCE OR COMPLETION IN VARIOUS DATA SETS, BY SPECIFIED INTERVALS POSTPARTUM

Name and Source of Data Set	9 Months Postpartum	12 Months Postpartum	15 Months Postpartum	18 Months Postpartum	24 Months Postpartum
Project Redirection	--	51% Experimentals 41% Controls	--	46% Experimentals 38% Controls	45% Experimentals 43% Controls
OAPP-Funded Projects (Burt *et al.*, 1984)	--	62%	--	--	58%
Young Mothers' Program (Klerman & Jekel, 1973; Jekel et al., 1973)	--	--	56% Experimentals 30% Controls	--	51% Experimentals 25% Controls
National Longitudinal Survey (Mott & Maxwell, 1981)	61% Blacks 58% Whites	--	--	--	--
Illinois AFDC Recipients (Testa, 1983)		42% (Estimate)			

was 12 months old or younger were in school at the time of the interview, compared with 39% whose child was older than a year. If one assumes that the 42% figure is a reasonable estimate for the 12-month postpartum rate, and if one takes into account that only 13% of this sample was Hispanic (Hispanics in Testa's study were less than half as likely as blacks to be in school), then one might expect that the school attendance rate for a subgroup of Testa's sample matched by ethnicity to the Redirection sample would be in the vicinity of 40% or lower. In other words, in an ethnically-matched group, the percentage with a positive school status would probably be somewhat lower than that for the Redirection comparison teens, and substantially lower than that for the experimental teens. This conclusion lends further credence to the internal validity of the 12-month impacts but, unfortunately sheds no further light on the 24-month impacts.

Employment-Related Outcomes

Data regarding the employment experiences of teen mothers are relatively uncommon. Table D.5 summarizes the three sources for which there is information for fixed intervals after a delivery. Once again, direct comparisons are problematic either because Project Redirection teens are younger than the reference group (e.g. Burt *et al*., 1984), because they are more disadvantaged and have less education (Burt *et al*., 1984; Furstenberg, 1976), or because the data were collected during a period of relatively high employment (Klerman and Jekel, 1973; Furstenberg, 1976). Consistent with these differences, the rates of employment among both experimental and comparison group teens in the Redirection sample were lower than that for teens in any other sample. At 24 months postpartum, however, the employment rate of OAPP clients was

TABLE D.5

COMPARISON OF EMPLOYMENT IN VARIOUS DATA SETS, BY SPECIFIED INTERVALS POSTPARTUM

Name and Source of Data Set	12 Months Postpartum	15 Months Postpartum	18 Months Postpartum	24 Months Postpartum
Project Redirection	13% Experimentals 15% Controls		18% Experimentals 18% Controls	18% Experimentals 20% Controls
OAPP-Funded Projects (Burt et al., 1984)	20%			22%
Young Mothers' Program (Klerman & Jekel, 1978; also Jekel et al., 1973)		31% Experimentals		32% Experimentals
Sinai Hospital Prenatal Clinic (Furstenberg, 1976)	25%			

similar to that for the younger and more disadvantaged Redirection sample.

Data from two other surveys provide additional information regarding the employment of teen mothers, although in neither case was there information about employment at fixed periods postpartum. In the sample of Illinois AFDC recipients, 25% of the teens were in the labor force at the time of the interview, but of these only 5% were actually employed (Testa, 1983). Thus, among these poor, mostly minority teens interviewed in the early 1980s, the percent employed was under 2%.

Using data from the National Longitudinal Survey of Labor Market Experiences of Youth, Mott and Maxwell (1981) reported that among the young black mothers who had not completed high school, the percent employed in 1979 was 7.5%. The rate for white dropouts was 26.2%. These two sources suggest that, given their youth, their academic credentials, and the economic conditions, both the experimental and comparison teens in the Redirection sample were probably doing as well as could be expected.

Conclusions

The review of other data sources does not lead to any clearcut revelations, given the differences in the age, ethnic mix, and poverty levels in the various samples. Nevertheless, this analysis failed to undermine the general conclusion that Project Redirection had numerous short-term impacts--absolute as well as incremental--on its clients. However, ambigiuty remains concerning longer-term impacts.

APPENDIX E

SUPPLEMENTARY TABLES

TABLE E.1

COMPARISON OF TEENS WHO WERE PREGNANT OR MOTHERS AT BASELINE ON SELECTED BASELINE VARIABLES

Variable	Percentages or Means, by Parenting Status		
	Pregnant Teens	Teen Mothers	All Teens
• Demographic			
Mean Age	16.3	16.6	16.4
Percent Married	7.2	5.0	6.4
Percent Black	43.4	51.2	46.4*
Percent Mexican American	25.2	22.9	24.3
Percent Puerto Rican	21.6	11.2	17.6**
Percent White	7.2	11.2	8.7*
• Educational			
Percent in School/GED Program	61.9	45.7	55.7***
Mean Highest Grade Completed	8.8	8.9	8.8
Mean Number of Times Dropped Out of School	0.6	0.8	0.7***
Mean Number of Semesters in a Teen Parent Program	0.4	0.6	0.5***
Percent in a School Teen Parent Program at Baseline	26.6	14.7	22.1***
• Family Planning/Fertility			
Mean Number of Pregnancies	1.2	1.2	1.2
Percent With One or More Abortions	4.3	6.6	5.2
Mean Age at First Birth	16.2	15.8	16.1**
Percent Ever Used Birth Control	32.7	62.2	43.8***
Percent Ever Used the Pill	19.9	42.2	28.4***
• Employment			
Percent Employed	8.9	10.9	9.6
Percent Ever Worked	59.0	67.1	62.1*
Mean Number of Jobs Held	0.9	1.1	1.0*
• Home Environment			
Percent in an AFDC Household	60.9	72.5	65.3**
Percent Raised by Both Parents	22.1	25.2	23.3
Percent Whose Mother was a Teen Mother	67.9	65.1	66.8
Mean Number of Siblings	5.1	5.0	5.0
Percent Whose Mother Completed High School/GED	28.5	29.8	29.0
Percent Whose Father Completed High School/GED	27.1	22.9	25.6
Number of Respondents	417	258	675

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members at baseline (Sample I) and 12-months post baseline (Sample II).

*The group difference is statistically significant at the .05 level.

**The group difference is statistically significant at the .01 level.

***The group difference is statistically significant at the .001 level.

TABLE E.2

COMPARISON OF YOUNGER AND OLDER TEENS ON SELECTED BASELINE VARIABLES

Variable	Percentages or Means, by Age Group		
	≤ 15 at Baseline	16-17 at Baseline	All Teens
● Demographic			
Percent Married	4.5	7.3	6.4
Percent Pregnant	69.5	58.0	61.8**
Percent Black	49.8	44.7	46.4
Percent Mexican American	23.8	24.6	24.3
Percent Puerto Rican	17.5	17.7	17.6
Percent White	7.2	9.5	8.7
● Educational			
Percent in School/GED Program	59.2	54.0	55.7
Mean Highest Grade Completed	8.1	9.2	8.8***
Mean Number of Times Dropped Out of School	0.6	0.7	0.7
Mean Number of Semesters in a Teen Parent Program	0.4	0.5	0.5
Percent in a School Teen Parent Program at Baseline	23.8	21.2	22.1
● Family Planning/Fertility			
Mean Number of Pregnancies	1.1	1.2	1.2***
Percent With One or More Abortions	2.7	6.4	5.2
Mean Age at First Birth	14.9	16.6	16.1***
Percent Ever Used Birth Control	39.1	46.1	43.8
Percent Ever Used the Pill	26.0	29.6	28.4
● Employment			
Percent Employed	4.9	11.9	9.6**
Percent Ever Worked	44.8	70.6	62.1***
Mean Number of Jobs Held	0.7	1.2	1.0***
● Home Environment			
Percent in an AFDC Household	65.9	65.0	65.3
Percent Raised by Both Parents	17.9	25.9	23.3*
Percent Whose Mother was a Teen Mother	72.2	64.2	66.8*
Mean Number of Siblings	5.0	5.1	5.0
Percent Whose Mother Completed High School/GED	28.7	29.2	29.0
Percent Whose Father Completed High School/GED	27.4	24.6	25.6
Number of Respondents	223	452	675

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members at baseline (Sample I) and 12-months post baseline (Sample II).

*The group difference is statistically significant at the .05 level.

**The group difference is statistically significant at the .01 level.

***The group difference is statistically significant at the .001 level.

TABLE E.3

COMPARISON OF TEENS IN OR OUT OF SCHOOL AT BASELINE ON SELECTED BASELINE VARIABLES

Variable	Percentage or Means, by School Status		
	In School/GED Program	Not in School /GED Program	All Teens
• Demographic			
Mean Age	16.3	16.5	16.4
Percent Married	3.5	10.0	6.4***
Percent Pregnant	68.6	53.2	61.8***
Percent Black	57.7	32.1	46.4***
Percent Mexican American	20.7	28.8	24.3*
Percent Puerto Rican	10.4	26.8	17.6***
Percent White	9.3	8.0	8.7
• Educational			
Mean Highest Grade Completed	8.9	8.7	8.8*
Mean Number of Times Dropped Out of School	0.3	1.1	0.7***
Mean Number of Semesters in a Teen Parent Program	0.6	0.3	0.5***
Percent in a School Teen Parent Program at Baseline	37.8	0.0	22.1***
• Family Planning/Fertility			
Mean Number of Pregnancies	1.1	1.3	1.2***
Percent With One or More Abortions	6.1	4.0	5.2
Mean Age at First Birth	16.2	15.9	16.1
Percent Ever Used Birth Control	42.4	45.5	43.8
Percent Ever Used the Pill	24.5	33.4	28.4*
• Employment			
Percent Employed	12.0	6.7	9.6*
Percent Ever Worked	66.5	56.5	62.1**
Mean Number of Jobs Held	1.1	0.9	1.0*
• Home Environment			
Percent in an AFDC Household	63.8	67.2	65.3
Percent Raised by Both Parents	25.0	21.1	23.3
Percent Whose Mother was a Teen Mother	67.0	66.6	66.8
Mean Number of Siblings	4.8	5.4	5.0**
Percent Whose Mother Completed High School/GED	35.6	20.7	29.0**
Percent Whose Father Completed High School/GED	30.3	19.4	25.6**
Number of Respondents	376	299	675

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members at baseline (Sample I) and 12-months post baseline (Sample II).

*The group difference is statistically significant at the .05 level.

**The group difference is statistically significant at the .01 level.

***The group difference is statistically significant at the .001 level.

TABLE E.4

COMPARISON OF TEENS WITH OR WITHOUT WORK EXPERIENCE AT BASELINE ON SELECTED BASELINE VARIABLES

Variable	Percentages or Means, by Work Experience		
	Had Work Experience	Had No Work Experience	All Teens
• Demographic			
Mean Age	16.7	16.2	16.4
Percent Married	6.9	5.5	6.4
Percent Pregnant	58.7	66.8	61.8
Percent Black	46.8	45.7	46.4
Percent Mexican American	24.1	24.6	24.3
Percent Puerto Rican	16.0	20.3	17.6
Percent White	9.8	7.0	8.7
• Educational			
Percent in School/GED Program	59.7	49.2	55.7**
Mean Highest Grade Completed	9.0	8.5	8.8***
Mean Number of Times Dropped Out of School	0.6	0.7	0.7
Mean Number of Semesters in a Teen Parent Program	0.5	0.3	0.5**
Percent in a School Teen Parent Program at Baseline	24.6	18.0	22.1*
• Family Planning/Fertility			
Mean Number of Pregnancies	1.2	1.2	1.2
Percent With One or More Abortions	7.2	2.0	5.2**
Mean Age at First Birth	16.4	15.5	16.1***
Percent Ever Used Birth Control	47.0	38.6	43.8
Percent Ever Used the Pill	29.4	27.0	28.4
• Employment			
Percent Employed	15.5	0.0	9.6***
Mean Number of Jobs Held	1.6	0.0	1.0***
• Home Environment			
Percent in an AFDC Household	67.8	61.3	65.3
Percent Raised by Both Parents	25.1	20.3	23.3
Percent Whose Mother was a Teen Mother	66.3	67.6	66.8
Mean Number of Siblings	5.0	5.0	5.0
Percent Whose Mother Completed High School/GED	31.0	25.8	29.0
Percent Whose Father Completed High School/GED	26.0	24.6	25.6
Number of Respondents	419	256	675

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members at baseline (Sample I) and 12-months post baseline (Sample II).

*The group difference is statistically significant at the .05 level.

**The group difference is statistically significant at the .01 level.

***The group difference is statistically significant at the .001 level.

TABLE E.5

COMPARISON OF TEENS IN OR NOT IN AN AFDC HOUSEHOLD AT BASELINE, ON SELECTED BASELINE VARIABLES

Variable	Percentages or Means, by Age Group		
	In an AFDC Household	Not in an AFDC Household	All Teens
• Demographic			
Mean Age	16.4	16.4	16.4
Percent Married	4.1	10.7	6.4***
Percent Pregnant	57.6	70.0	61.9**
Percent Black	48.1	42.9	46.3
Percent Mexican American	18.6	35.2	24.3**
Percent Puerto Rican	22.0	9.4	17.7**
Percent White	7.5	11.2	8.8*
• Educational			
Percent in School/GED Program	54.4	58.4	55.8
Mean Highest Grade Completed	8.8	8.9	8.8+
Mean Number of Times Dropped Out of School	0.7	0.6	0.7
Mean Number of Semesters in a Teen Parent Program	0.5	0.4	0.4*
Percent in a School Teen Parent Program at Baseline	22.4	21.5	22.1
• Family Planning/Fertility			
Mean Number of Pregnancies	1.2	1.1	1.2***
Percent With One or More Abortions	5.9	3.9	5.2
Mean Age at First Birth	16.1	16.1	16.1
Percent Ever Used Birth Control	48.0	35.9	43.8
Percent Ever Used the Pill	34.0	18.0	28.5***
• Employment			
Percent Employed	8.6	11.6	9.6
Percent Ever Worked	64.4	57.9	62.2
Mean Number of Jobs Held	1.1	0.9	1.0*
• Home Environment			
Percent Raised by Both Parents	17.7	33.9	23.3***
Percent Whose Mother was a Teen Mother	69.8	61.4	66.9*
Mean Number of Siblings	5.2	4.8	5.0+
Percent Whose Mother Completed High School/GED	26.5	33.9	29.0*
Percent Whose Father Completed High School/GED	23.4	29.6	25.6+
Number of Respondents	441	224	675

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members at baseline (Sample I) and 12-months post baseline (Sample II).

*The group difference is statistically significant at the .05 level.

**The group difference is statistically significant at the .01 level.

***The group difference is statistically significant at the .001 level.

TABLE E.6

LOGISTIC REGRESSION OF A PREGNANCY SUBSEQUENT TO THE INDEX PREGNANCY BY THE 24-MONTH INTERVIEW, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	(1)		(2)		(3)	
Age at First Birth	- .05	(.09)	- .05	(.09)	- .04	(.09)
White	.02	(.33)	.00	(.33)	.01	(.33)
Black	.28	(.21)	.30	(.22)	.30	(.22)
Puerto Rican	.28	(.25)	.28	(.26)	.31	(.26)
Married	.02	(.36)	.01	(.36)	.04	(.35)
Mother's Education	- .08*	(.04)	- .08*	(.04)	- .08*	(.04)
In School or GED Program	- .43*	(.21)	- .45*	(.21)	- .42*	(.21)
Highest Grade Completed	- .03	(.09)	- .03	(.09)	- .03	(.09)
Number of Times Dropped Out of School	.31*	(.14)	.30*	(.14)	.31*	(.14)
Number of Semesters in a Teen Parent Program	.17	(.12)	.17	(.12)	.17	(.12)
Pregnant at Baseline	.25	(.24)	.24	(.24)	.26	(.24)
Number of Baseline Pregnancies	- .05	(.19)	- .06	(.19)	- .03	(.19)
Ever Used Oral Contraceptives	.18	(.19)	.17	(.19)	.16	(.19)
Number of Months at Risk to a Pregnancy at Baseline	.05**	(.02)	.05**	(.02)	.05**	(.02)
Number of Months Between Baseline and 24-Month Interview	.13***	(.04)	.14***	(.04)	.13***	(.03)
Participated in Project Redirection	- .19	(.19)	—		—	
Number of Months Participated in Project Redirection	—		- .02+	(.01)	—	
Received Birth Control Counseling From Project Redirection	—		—		- .44	(.30)
Received Birth Control Counseling Elsewhere Since Baseline	—		—		.07	(.20)
Constant	-2.76		-2.86		-2.90	
Number of Respondents	660		660		660	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]Participation was measured in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of birth control counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[b]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c]Unless otherwise specified, all explanatory variables are baseline characteristics.

[d]The coefficients shown are standardized coefficients (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
***Statistically significant at the .001 level.

TABLE E.7

LOGISTIC REGRESSION OF A LIVE BIRTH SUBSEQUENT TO THE INDEX PREGNANCY BY 24 MONTHS POST-BASELINE, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	(1)		(2)		(3)	
Age at First Birth	- .08	(.10)	- .08	(.10)	- .07	(.10)
White	- .62	(.41)	- .64	(.41)	- .63	(.41)
Black	- .10	(.24)	- .08	(.24)	- .09	(.24)
Puerto Rican	.08	(.29)	.08	(.29)	.13	(.29)
Married	.11	(.39)	.10	(.39)	.17	(.39)
Mother's Education	- .02	(.05)	- .02	(.05)	- .02	(.05)
In School or GED Program	- .64**	(.24)	- .65**	(.24)	- .57*	(.24)
Highest Grade Completed	- .01	(.10)	- .01	(.10)	- .02	(.10)
Number of Times Dropped Out of School	- .01	(.16)	- .03	(.16)	- .00	(.16)
Number of Semesters in a Teen Parent Program	- .26+	(.14)	- .25+	(.14)	- .23+	(.14)
Pregnant at Baseline	.16	(.27)	.14	(.27)	.18	(.27)
Number of Baseline Pregnancies	- .18	(.22)	- .20	(.22)	- .16	(.22)
Ever Used Oral Contraceptives	.07	(.21)	.05	(.21)	.06	(.21)
Number of Months at Risk to a Pregnancy at Baseline	.06***	(.02)	.06***	(.02)	.06***	(.02)
Number of Months Between Baseline and 24-Month Interview	.14***	(.04)	.15***	(.04)	.12***	(.04)
Participated in Project Redirection	- .36+	(.22)	—		—	
Number of Months Participated in Project Redirection	—		- .03*	(.02)	—	
Received Birth Control Counseling From Project Redirection	—		—		- .49	(.36)
Received Birth Control Counseling Elsewhere Since Baseline	—		—		- .13	(.22)
Constant	-2.68		-2.85		-2.40	
Number of Respondents	660		660		660	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]Participation was measured in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of birth control counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[b]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c]Unless otherwise specified, all explanatory variables are baseline characteristics.

[d]The coefficients shown are standardized coefficients (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.8

ORDINARY LEAST SQUARES REGRESSION OF NUMBER OF PREGNANCIES AT 24-MONTH INTERVIEW, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	(1)		(2)		(3)	
Age at First Birth	-.02+	(.01)[d]	-.02+	(.01)	-.02+	(.01)
White	-.02	(.07)	-.02	(.07)	-.02	(.07)
Black	.03	(.05)	.04	(.05)	.04	(.05)
Puerto Rican	-.00	(.06)	-.01	(.06)	-.01	(.06)
Married	.08	(.08)	.07	(.08)	.07	(.08)
Number of Siblings	.01	(.01)	.01	(.01)	.01	(.01)
In School or GED Program	.04	(.05)	.03	(.05)	.03	(.05)
Highest Grade Completed	.01	(.02)	.01	(.02)	.01	(.02)
Number of Times Dropped out of School	.14***	(.03)	.14***	(.03)	.14***	(.03)
Number of Semesters in a Teen Parent Program	.09***	(.03)	.09***	(.03)	.09***	(.03)
Pregnant at Baseline	-.47***	(.04)	-.47***	(.04)	-.47***	(.04)
Number of Pregnancies at Baseline	.82***	(.04)	.82***	(.04)	.82***	(.04)
Ever Used Oral Contraceptives	.07	(.04)	.07	(.04)	.06	(.04)
Days at Risk to a Post-Baseline Pregnancy	-.01***	(.00)	-.00***	(.00)	-.00***	(.00)
Number of Months Between Baseline and 24-Month Interview	.05***	(.01)	.05***	(.01)	.06***	(.01)
Participated in Project Redirection	.04	(.04)	—		—	
Number of Months Participated in Project Redirection	—		-.01	(.00)	—	
Received Birth Control Counseling From Project Redirection	—		—		-.11+	(.06)
Received Birth Control Counseling Elsewhere Since Baseline	—		—		-.01	(.04)
Constant	0.66		0.61		0.58	
Adjusted R^2	.63		.63		.63	
Number of Respondents	662		662		662	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]Participation was measured in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of birth control counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[b]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c]Unless otherwise specified, all explanatory variables are baseline characteristics.

[d]The coefficients shown are unstandardized coefficients (b's). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
***Statistically significant at the .001 level.

TABLE E.9

TOBIT REGRESSION OF INTERVAL BETWEEN INDEX AND SUBSEQUENT PREGNANCIES, ON BACKGROUND CHARACTERESTICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	Unstandardized Coefficient	Standard Error
Age at First Birth	- .29	.34
White	.72	1.47
Black	- .88	.94
Puerto Rican	-1.06	1.15
Married	- .24	1.56
Number of Siblings	- .16	.12
In School or GED Program	2.88***	.85
Number of Semesters in a Teen Parent Program	.55	1.04
Number of Baseline Pregnancies	- .34	.85
Ever Used Any Contraception	- .45	.74
Time Between Termination of Index Pregnancy and Baseline	.69***	.06
Participated in Project Redirection	1.81*	.78
Constant	23.62	
Number of Respondents	660	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Tobit regression, rather than ordinary least squares regression, was used because of truncation. That is, it was not always possible to ascertain during the study period the length of time between the index pregnancy and a subsequent one because for some teens a subsequent pregnancy had not occurred. Tobit analysis adjusts for such truncated or censored cases.

[b] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c] Unless otherwise specified, all explanatory variables are baseline characteristics.

*Statistically significant at the .05 level.
***Statistically significant at the .001 level.

TABLE E.10

LOGISTIC REGRESSION OF A PREGNANCY WITHIN 12 MONTHS SUBSEQUENT TO THE INDEX PREGNANCY, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	(1)		(2)		(3)	
Age at First Birth	.09	(.10)[d]	.10	(.10)	.10	(.10)
White	- .25	(.44)	- .25	(.44)	- .24	(.44)
Black	.34	(.26)	.35	(.26)	.35	(.26)
Puerto Rican	.42	(.28)	.43	(.28)	.45	(.28)
Married	.14	(.38)	.15	(.38)	.17	(.38)
Number of Siblings	.03	(.03)	.03	(.03)	.03	(.03)
In School or GED Program	- .97***	(.23)	- .96***	(.23)	- .94***	(.23)
Highest Grade Completed	- .14	(.1)	- .14	(.11)	- .14	(.11)
Number of Semesters in a Teen Parent Program	.04	(.16)	.05	(.16)	.06	(.16)
Number of Baseline Pregnancies	.02	(.22)	.01	(.22)	.03	(.22)
Ever Used Oral Contraceptives	- .00	(.23)	- .01	(.23)	- .02	(.23)
Number of Months Between Baseline and Index Pregnancy	- .08***	(.02)	- .08***	(.02)	- .08***	(.02)
Participated in Project Redirection	- .22	(.21)	—		—	
Number of Months Participated in Project Redirection	—		- .02	(.01)	—	
Received Birth Control Counseling From Project Redirection	—		—		- .43	(.38)
Received Birth Control Counseling Elsewhere Since Baseline	—		—		- .09	(.23)
Constant	-2.29		-2.34		-2.41	
Number of Respondents	652		652		652	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Participation was measured in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of birth control counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[b] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c] Unless otherwise specified, all explanatory variables are baseline characteristics.

[d] The coefficients shown are standardized coefficients (Betas). The numbers in parentheses are the standard errors.

TABLE E.11

LOGISTIC REGRESSION OF A PREGNANCY WITHIN 18 MONTHS SUBSEQUENT TO THE INDEX PREGNANCY, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	(1)		(2)		(3)	
Age at First Birth	.02	(.10)[d]	.03	(.09)	.04	(.09)
White	- .10	(.36)	- .10	(.36)	- .09	(.36)
Black	.25	(.23)	.27	(.23)	.25	(.23)
Puerto Rican	.27	(.27)	.29	(.27)	.31	(.27)
Married	.05	(.37)	.06	(.36)	.10	(.36)
Number of Siblings	.03	(.03)	.03	(.03)	.03	(.03)
In School or GED Program	- .90***	(.20)	- .88***	(.20)	- .86***	(.20)
Highest Grade Completed	- .03	(.10)	- .04	(.10)	- .04	(.10)
Number of Semesters in a Teen Parent Program	- .08	(.14)	- .06	(.14)	- .06	(.14)
Number of Baseline Pregnancies	- .11	(.20)	- .12	(.20)	- .09	(.20)
Ever Used Oral Contraceptives	.20	(.20)	.19	(.20)	.18	(.20)
Number of Months Between Baseline and Index Pregnancy	- .10***	(.02)	- .10***	(.02)	- .10***	(.02)
Participated in Project Redirection	- .32*	(.18)	—		—	
Number of Months Participated in Project Redirection	—		- .02+	(.01)	—	
Received Birth Control Counseling From Project Redirection	—		—		- .45	(.32)
Received Birth Control Counseling Elsewhere Since Baseline	—		—		.10	(.21)
Constant	- .27		- .38		- .80	
Number of Respondents	604		604		604	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]Participation was measured in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of birth control counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[b]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c]Unless otherwise specified, all explanatory variables are baseline characteristics.

[d]The coefficients shown are standardized coefficients (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
***Statistically significant at the .001 level.

TABLE E.12

LOGISTIC REGRESSION OF A PREGNANCY WITHIN 24 MONTHS SUBSEQUENT TO THE INDEX PREGNANCY, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	(1)		(2)		(3)	
Age at First Birth	.07	(.12)[d]	.07	(.12)	.10	(.11)
White	- .53	(.40)	- .55	(.40)	- .57	(.40)
Black	.28	(.27)	.31	(.27)	.24	(.27)
Puerto Rican	.54	(.36)	.55	(.36)	.58+	(.36)
Married	.16	(.49)	.16	(.49)	.20	(.48)
Number of Siblings	.03	(.04)	.02	(.04)	.03	(.04)
In School or GED Program	- .30	(.24)	- .32	(.24)	- .26	(.24)
Highest Grade Completed	- .14	(.11)	- .14	(.11)	- .14	(.11)
Number of Semesters in a Teen Parent Program	- .10	(.15)	- .09	(.15)	- .13	(.15)
Number of Baseline Pregnancies	.09	(.25)	.07	(.25)	.14	(.25)
Ever Used Oral Contraceptives	.26	(.24)	.23	(.24)	.23	(.24)
Number of Months Between Baseline and Index Pregnancy	- .14***	(.02)	- .14***	(.02)	- .14***	(.02)
Participated in Project Redirection	- .30	(.23)	—		—	
Number of Months Participated in Project Redirection	—		- .03*	(.01)	—	
Received Birth Control Counseling From Project Redirection	—		—		- .20	(.37)
Received Birth Control Counseling Elsewhere Since Baseline	—		—		.47+	(.25)
Constant	- .45		- .36		-1.39	
Number of Respondents	475		475		475	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Participation was measured in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of birth control counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[b] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c] Unless otherwise specified, all explanatory variables are baseline characteristics.

[d] The coefficients shown are standardized coefficients (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
***Statistically significant at the .001 level.

TABLE E.13

LOGISTIC REGRESSION OF USE OF A MEDICALLY PRESCRIBED CONTRACEPTIVE BY 24-MONTH INTERVIEW[a], ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1)		(2)		(3)	
Age	- .23*	(.10)[e]	- .23*	(.10)	- .22	(.10)
White	.28	(.40)	.28	(.40)	.19	(.42)
Black	- .03	(.26)	- .04	(.25)	- .11	(.26)
Puerto Rican	.72*	(.36)	.70*	(.35)	.73*	(.36)
Married	.22	(.45)	.20	(.43)	.16	(.45)
Raised by Both Parents	- .43+	(.25)	- .44+	(.24)	- .43+	(.25)
Number of Siblings	.08*	(.04)	.08*	(.04)	.09*	(.04)
In School or GED Program	.25	(.24)	.23	(.24)	.18	(.24)
Number of Semesters in a Teen Parent Program	.51**	(.19)	.50**	(.18)	.44*	(.19)
Pregnant at Baseline	- .33	(.23)	- .32	(.23)	- .34	(.24)
Number of Baseline Pregnancies	.39	(.27)	.39	(.27)	.42	(.28)
Ever Used Oral Contraceptives	.36+	(.21)	.36+	(.21)	.42*	(.21)
Ever Employed	.19	(.22)	.18	(.22)	.15	(.22)
Number of Months Between Baseline and 24-Month Interview	- .01	(.05)	- .00	(.05)	.01	(.04)
Participated in Project Redirection	.18	(.24)	—		—	
Number of Months Participated in Project Redirection	—		.01	(.02)	—	
Received Birth Control Counseling From Project Redirection	—		—		.70+	(.39)
Received Birth Control Counseling Elsewhere Since Baseline	—		—		1.21***	(.23)
Constant	4.16		4.05		2.69	
Number of Respondents	656		656		656	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Teens who had ever used oral contraceptives, the diaphragm, or the IUD were coded 1, others were coded 0.

[b] Participation was measured in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of birth control counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[c] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d] Unless otherwise specified, all explanatory variables are baseline characteristics.

[e] The coefficients shown are standardized coefficients (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.14

ORDINARY LEAST SQUARES REGRESSION OF BIRTH CONTROL KNOWLEDGE TEST SCORES[a] FOR SAMPLE I TEENS AT 24-MONTH INTERVIEW, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1)		(2)		(3)	
Age at First Birth	.00	(.06)[e]	.01	(.06)	.00	(.06)
White	.94+	(.49)	.99*	(.49)	.88+	(.49)
Black	.01	(.32)	- .00	(.33)	- .01	(.32)
Puerto Rican	-1.67***	(.44)	-1.70***	(.43)	-1.76***	(.43)
Married	.23	(.55)	.17	(.54)	.13	(.54)
Raised by Both Parents	.61*	(.31)	.58+	(.31)	.58+	(.31)
Number of Siblings	- .06	(.04)	- .06	(.04)	- .06	(.04)
In School or GED Program	.33	(.31)	.31	(.31)	.23	(.30)
Highest Grade Completed	.12	(.13)	.14	(.13)	.14	(.13)
Number of Semesters in a Teen Parent Program	.21	(.18)	.17	(.18)	.10	(.18)
Pregnant at Baseline	.28	(.29)	.33	(.29)	.32	(.29)
Number of Pregnancies	- .62*	(.30)	- .63*	(.30)	- .73*	(.29)
Ever Used Contraceptives	.41	(.32)	.43	(.32)	.64*	(.32)
Ever Used Oral Contraceptives	- .32	(.36)	- .30	(.36)	- .45	(.36)
Baseline Score on Birth Control Knowledge Test	.37***	(.04)	.36***	(.05)	.36***	(.05)
Participated in Project Redirection	.57*	(.28)	—		—	
Number of Months Participated in Project Redirection	—		.04*	(.02)	—	
Received Birth Control Counseling From Project Redirection	—		—		1.30**	(.46)
Received Birth Control Counseling Elsewhere Since Baseline	—		—		.85**	(.31)
Constant	7.0		7.0		6.6	
Adjusted R^2	.33		.33		.34	
Number of Respondents	359		359		359	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I only at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] The Birth Control Knowledge Test is a 16-item test designed to measure knowledge about various contraceptive methods and risk of pregnancy. Scores could range from 0 (no correct answers) to 16 (all correct answers).

[b] Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of birth control counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[c] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d] Unless otherwise specified, all explanatory variables are baseline characteristics.

[e] The coefficients shown are unstandardized (b's). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.15

LOGISTIC REGRESSION OF POSITIVE SCHOOL STATUS[a] AT 24 MONTH INTERVIEW, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1)		(2)		(3)	
Age at First Birth	- .28**	(.09)[e]	- .26**	(.09)	- .27**	(.10)
White	1.09**	(.37)	1.12**	(.37)	1.15**	(.38)
Black	.75***	(.24)	.73**	(.24)	.76**	(.24)
Puerto Rican	- .36	(.32)	- .34	(.32)	- .32	(.32)
Married	- .48	(.43)	- .40	(.43)	- .23	(.44)
Mother's Education	- .05	(.05)	- .04	(.05)	- .05	(.05)
Father's Education	.06+	(.04)	.07+	(.04)	.07+	(.04)
Raised by Both Parents	.08	(.23)	.08	(.23)	.09	(.23)
Number of Siblings	- .04	(.03)	- .03	(.03)	- .03	(.03)
In School or GED Program	.80***	(.24)	.89***	(.24)	.93***	(.24)
Highest Grade Completed	.37***	(.10)	.36***	(.10)	.36***	(.10)
Number of Times Dropped Out of School	- .39*	(.16)	- .38*	(.16)	- .36*	(.16)
Enrolled in a Teen Parent Program	.43+	(.23)	.40+	(.23)	.34	(.24)
Number of Baseline Pregnancies	- .31	(.25)	- .28	(.25)	- .23	(.26)
Ever Had an Abortion	.64	(.45)	.64	(.45)	.62	(.45)
Employed at Baseline	.48	(.33)	.52	(.33)	.51	(.33)
Number of Baseline Jobs	.21*	(.10)	.19*	(.10)	.17+	(.10)
Participated in Project Redirection	- .13	(.19)	—		—	
Number of Months Participated in Project Redirection	—		.02	(.01)	—	
Received Educational Counseling From Project Redirection	—		—		.66**	(.25)
Received Educational Counseling Elsewhere Since Baseline	—		—		.79***	(.22)
Constant	2.82		2.27		2.04	
Number of Respondents	654		654		654	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Teens who were either enrolled in an educational program or had received a diploma or GED certificate were defined as having a positive school status.

[b] Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of educational counseling from the program was included, coded 1 if recieved from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[c] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d] Unless otherwise specified, all explanatory variables are baseline characteristics.

[e] The coefficients shown are standardized coefficient (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.16

LOGISTIC REGRESSION OF SCHOOL COMPLETION[a] AT 24 MONTH INTERVIEW, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1)		(2)		(3)	
Age at First Birth	.02	(.12)[e]	.05	(.13)	.03	(.12)
White	.55	(.41)	.63	(.41)	.66	(.42)
Black	- .19	(.31)	- .17	(.31)	- .12	(.31)
Puerto Rican	- .42	(.42)	- .31	(.42)	- .27	(.41)
Married	- .20	(.51)	- .10	(.51)	- .06	(.51)
Mother's Education	.11*	(.06)	.11*	(.06)	.11*	(.06)
Father's Education	- .01	(.05)	- .01	(.05)	- .00	(.05)
Raised by Both Parents	.29	(.27)	.28	(.27)	.30	(.27)
Number of Siblings	- .07+	(.04)	- .06	(.04)	- .06	(.04)
In School or GED Program	- .27	(.31)	- .17	(.31)	- .17	(.31)
Highest Grade Completed	.77***	(.13)	.75***	(.13)	.76***	(.13)
Number of Times Dropped Out of School	- .36+	(.20)	- .34+	(.20)	- .36+	(.20)
Enrolled in a Teen Parent Program	.60*	(.28)	.58*	(.28)	.56*	(.28)
Number of Baseline Pregnancies	- .06	(.32)	- .02	(.32)	.01	(.32)
Ever Had an Abortion	.99*	(.48)	.98*	(.48)	.97*	(.49)
Employed at Baseline	.69*	(.35)	.75*	(.36)	.74*	(.36)
Number of Baseline Jobs	.24*	(.11)	.22*	(.11)	.22*	(.11)
Number of Months Between Baseline and 24-Month Interview	.08	(.05)	.05	(.05)	.05	(.05)
Participated in Project Redirection	- .01	(.26)	—		—	
Number of Months Participated in Project Redirection	—		.02	(.02)	—	
Received Educational Counseling From Project Redirection	—		—		.61*	(.30)
Received Educational Counseling Elsewhere Since Baseline	—		—		.16	(.26)
Constant	-6.20		-6.26		-6.16	
Number of Respondents	654		654		654	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]Teens who either received their high school diploma or GED certificate were coded 1; those who had not completed basic schooling were coded 0.

[b]Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of educational counseling from the program was included, coded 1 if recieved from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The coefficients shown are standardized coefficients (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
***Statistically significant at the .001 level.

TABLE E.17

LOGISTIC REGRESSION OF TEENS ENROLLED IN SCHOOL OR GED PROGRAM POST BASELINE, ON BASELINE CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	(1)	(2)	(3)
Age at First Birth	- .24* (.12)[d]	- .23+ (.13)	- .21+ (.12)
White	.79 (.53)	.86 (.54)	.90+ (.54)
Black	.47 (.31)	.36 (.32)	.56+ (.32)
Puerto Rican	- .49 (.32)	- .60+ (.32)	- .58+ (.32)
Married	- .74+ (.42)	- .78+ (.42)	- .70+ (.41)
Mother's Education	- .04 (.07)	- .04 (.07)	- .05 (.07)
Father's Education	.05 (.06)	.06 (.06)	.03 (.06)
Raised by Both Parents	.03 (.30)	- .06 (.31)	.07 (.31)
Number of Siblings	- .06 (.04)	- .05 (.04)	- .07+ (.04)
In School or GED Program	2.39*** (.35)	2.41*** (.35)	2.13*** (.33)
Highest Grade Completed	.77*** (.13)	.75*** (.13)	.76*** (.13)
Number of Times Dropped Out of School	- .24 (.19)	- .22 (.20)	- .27 (.19)
Enrolled in a Teen Parent Program	.45 (.46)	.38 (.46)	.31 (.45)
Number of Baseline Pregnancies	- .54* (.27)	- .46+ (.28)	- .47+ (.27)
Ever Had an Abortion	.20 (.62)	.24 (.63)	.22 (.64)
Employed at Baseline	.55 (.54)	.66 (.55)	.56 (.55)
Number of Baseline Jobs	.35** (.13)	.29* (.13)	.30* (.14)
Participated in Project Redirection	1.31*** (.26)	—	—
Number of Months Participated in Project Redirection	—	.13*** (.02)	—
Received Educational Counseling From Project Redirection	—	—	1.75*** (.39)
Received Educational Counseling Elsewhere Since Baseline	—	—	1.26*** (.38)
Constant	3.89	3.72	3.83
Number of Respondents	654	654	654

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Teens who had ever enrolled in an educational program post-baseline were coded 1; all others were coded 0.

[b] Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of educational counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[c] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d] Unless otherwise specified, all explanatory variables are baseline characteristics.

[e] The coefficients shown are standardized coefficients (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.18

ORDINARY LEAST SQUARES REGRESSION OF NUMBER OF SEMESTERS IN SCHOOL POST-BASELINE, ON BACKGROUND CHARACTERITICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	(1)		(2)		(3)	
Age at First Birth	- .04	(.03)[d]	- .04+	(.03)	- .04+	(.03)
White	.33	(.23)	.38+	(.22)	.35	(.22)
Black	.37*	(.15)	- .33*	(.14)	.38**	(.14)
Puerto Rican	- .44*	(.18)	- .42*	(.18)	- .42*	(.18)
Married	- .52*	(.24)	- .49*	(.24)	- .48*	(.23)
Raised by Both Parents	.19*	(.14)	.16	(.14)	.17	(.14)
Number of Siblings	- .01	(.02)	- .01	(.02)	- .01	(.02)
Mother's Education	.01	(.01)	.01	(.01)	.01	(.01)
Father's Education	.00	(.02)	.00	(.02)	.00	(.01)
In School or GED Program	.83***	(.15)	.88***	(.15)	.82***	(.15)
Highest Grade Completed	- .09	(.06)	- .09	(.06)	- .08	(.06)
Number of Times Dropped out of School	- .33***	(.10)	- .31**	(.10)	- .30**	(.10)
Enrolled in a Teen Parent Program	.13	(.15)	.11	(.15)	.08	(.15)
Number of Pregnancies	.05	(.13)	.09	(.13)	.05	(.14)
Employed at Baseline	.48*	(.21)	.53**	(.20)	.50*	(.20)
Number of Baseline Jobs Held	.17**	(.06)	.15**	(.06)	.14*	(.06)
Number of Months Between Baseline and 24-Month Interview	.03	(.02)	.01	(.02)	.04+	(.02)
Participated in Project Redirection	.47**	(.13)	—		—	
Number of Months Participated in Project Redirection	—		.05***	(.01)	—	
Received Educational Counseling from Project Redirection	—		—		.83***	(.15)
Received Educational Counseling Elsewhere Since Baseline	—		—		.53***	(.13)
Constant	1.13		1.58		0.65	
Adjusted R^2	.24		.27		.28	
Number of Respondents	651		651		651	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of educational counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[b]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c]Unless otherwise specified, all explanatory variables are baseline characteristics.

[d]The coefficients shown are unstandardized (b's). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.19

LOGISTIC REGRESSION OF POSITIVE SCHOOL STATUS 12 MONTHS AFTER TERMINATION OF INDEX PREGNANCY[a], ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1)	(2)	(3)
Age at First Birth	- .35*** (.10)[e]	- .35*** (.10)	- .38*** (.10)
White	.24 (.35)	.28 (.35)	.20 (.35)
Black	.47** (.22)	.44* (.23)	.48* (.23)
Puerto Rican	- .12 (.29)	- .13 (.29)	- .11 (.30)
Married	- .22 (.41)	- .20 (.41)	- .18 (.41)
Raised by Both Parents	.33 (.22)	.29 (.23)	.31 (.23)
Number of Siblings	- .04 (.03)	- .04 (.03)	- .04 (.03)
In School or GED Program	1.32*** (.23)	1.35*** (.23)	1.23*** (.22)
Highest Grade Completed	.18+ (.10)	.19+ (.10)	.20* (.10)
Number of Times Dropped Out of School	- .26+ (.15)	- .25 (.16)	- .23 (.16)
Number of Semesters in a Teen Parent Program	.13 (.13)	.11 (.13)	.10 (.13)
Number of Baseline Pregnancies	- .27 (.22)	- .24 (.22)	- .26 (.22)
Number of Months Between Baseline and Index Pregnancy	.02 (.01)	.02 (.01)	.02 (.01)
Employed at Baseline	.43 (.32)	.47 (.32)	.40 (.32)
Number of Baseline Jobs	.14 (.09)	.13 (.10)	.13 (.09)
Participated in Project Redirection	.51** (.19)	—	—
Number of Months Participated in Project Redirection	—	.05*** (.01)	—
Received Educational Counseling From Project Redirection	—	—	.50* (.24)
Received Educational Counseling Elsewhere Since Baseline	—	—	.66** (.21)
Constant	4.17	4.03	4.57
Number of Respondents	647	647	647

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Teens who were either enrolled in an educational program or had received a diploma or GED certificate were defined as having a positive school status.

[b] Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of educational counseling from the program was included, coded 1 if recieved from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[c] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d] Unless otherwise specified, all explanatory variables are baseline characteristics.

[e] The coefficients shown are standardized coefficient (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.20

LOGISTIC REGRESSION OF POSITIVE SCHOOL STATUS 18 MONTHS AFTER TERMINATION OF INDEX PREGNANCY[a], ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1)		(2)		(3)	
Age at First Birth	- .22*	(.11)	- .21*	(.11)	- .24*	(.11)
White	1.16**	(.38)	1.23**	(.39)	1.25***	(.39)
Black	.45+	(.24)	.41+	(.24)	.49*	(.25)
Puerto Rican	.06	(.33)	.05	(.33)	.12	(.33)
Married	- .19	(.46)	- .15	(.46)	- .10	(.46)
Raised by Both Parents	.26	(.24)	.22	(.22)	.26	(.25)
Number of Siblings	- .01	(.03)	- .01	(.03)	- .01	(.03)
In School or GED Program	1.29***	(.24)	1.36***	(.24)	1.32***	(.24)
Highest Grade Completed	.11	(.11)	.11	(.11)	.12	(.11)
Number of Times Dropped Out of School	- .34*	(.16)	- .33*	(.17)	- .33*	(.17)
Number of Semesters in a Teen Parent Program	.15	(.13)	.14	(.13)	.13	(.13)
Number of Baseline Pregnancies	- .07	(.22)	- .02	(.23)	- .03	(.23)
Number of Months Between Baseline and Index Pregnancy	.03+	(.02)	.03+	(.02)	.03+	(.02)
Employed at Baseline	.03	(.35)	.08	(.35)	.07	(.35)
Number of Baseline Jobs	.33***	(.10)	.32**	(.10)	.31**	(.10)
Participated in Project Redirection	.40*	(.20)	—		—	
Number of Months Participated in Project Redirection	—		.05***	(.01)	—	
Received Educational Counseling From Project Redirection	—		—		.95***	(.25)
Received Educational Counseling Elsewhere Since Baseline	—		—		.49*	(.23)
Constant	1.55		1.18		1.61	
Number of Respondents	573		573		573	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]Teens who were either enrolled in an educational program or had received a diploma or GED certificate were defined as having a positive school status.

[b]Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of educational counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The coefficients shown are standardized coefficient (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.21

LOGISTIC REGRESSION OF POSITIVE SCHOOL STATUS 24 MONTHS AFTER TERMINATION OF INDEX PREGNANCY[a], ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1)		(2)		(3)	
Age at First Birth	- .28*	(.13)[e]	- .27*	(.13)	- .28*	(.13)
White	.97*	(.46)	.99*	(.46)	.99*	(.47)
Black	.35	(.30)	.31	(.30)	.31	(.30)
Puerto Rican	- .27	(.42)	- .28	(.42)	- .20	(.42)
Married	.23	(.54)	.29	(.54)	.43	(.55)
Raised by Both Parents	- .19	(.29)	- .21	(.29)	- .18	(.29)
Number of Siblings	- .01	(.04)	- .00	(.03)	.01	(.04)
In School or GED Program	.55*	(.28)	.62*	(.28)	.72**	(.29)
Highest Grade Completed	.21	(.13)	.20	(.13)	.19	(.13)
Number of Times Dropped Out of School	- .51**	(.21)	- .50*	(.21)	- .52**	(.21)
Number of Semesters in a Teen Parent Program	.08	(.15)	.09	(.15)	.08	(.15)
Number of Baseline Pregnancies	- .05	(.27)	- .02	(.27)	.04	(.28)
Number of Months Between Baseline and Index Pregnancy	.04+	(.02)	.04*	(.02)	.04*	(.02)
Employed at Baseline	.27	(.42)	.28	(.42)	.25	(.43)
Number of Baseline Jobs	- .05	(.13)	- .02	(.13)	.10	(.13)
Participated in Project Redirection	- .05	(.25)	—		—	
Number of Months Participated in Project Redirection	—		.02	(.02)	—	
Received Educational Counseling From Project Redirection	—		—		.78**	(.30)
Received Educational Counseling Elsewhere Since Baseline	—		—		.48+	(.29)
Constant	3.39		3.03		2.90	
Number of Respondents	347		347		347	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Teens who were either enrolled in an educational program or had received a diploma or GED certificate were defined as having a positive school status.

[b] Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of educational counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[c] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d] Unless otherwise specified, all explanatory variables are baseline characteristics.

[e] The coefficients shown are standardized coefficient (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.

TABLE E.22

LOGISTIC REGRESSION OF SCHOOL/WORK STATUS[a] AT 24-MONTH INTERVIEW, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Standardized Coefficient (Beta)	Standard Error
Age	-.21*	.10
White	.99**	.36
Black	.58**	.22
Puerto Rican	-.56+	.30
Married	.23	.37
Age of Index Child	.02+	.02
In an AFDC Household	-.32	.20
Raised by Both Parents	.04	.22
Number of Siblings	.02	.03
In School or GED Program	.76***	.22
Highest Grade Completed	.28**	.10
Number of Times Dropped Out of School	-.31*	.15
Number of Semesters in a Teen Parent School Program	.26*	.13
Pregnant at Baseline	.15	.23
Employed at Baseline	.36	.33
Number of Baseline Jobs	.24**	.09
Date of Enrollment/Baseline	.00	.00
Participated in Project Redirection	.10	.19
Constant	107.4	
Number of Respondents	652	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Teens who were either attending an educational program or had completed their high school or GED program, or who were working at the final interview were coded 1; others were coded 0.

[b] Participation was coded 1 for experimental group teens, 0 for comparison group teens.

[c] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d] Unless otherwise specified, all explanatory variables are baseline characteristics.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.23

LOGISTIC REGRESSION OF SCHOOL/LABOR FORCE STATUS[a] AT 24-MONTH INTERVIEW, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Standardized Coefficient (Beta)	Standard Error
Age	- .29**	.11
White	1.25**	.44
Black	.78***	.24
Puerto Rican	- .99***	.28
Married	.26	.38
Age of Index Child	.03+	.02
In an AFDC Household	.11	.22
Raised by Both Parents	.02	.24
Number of Siblings	.03	.03
In School or GED Program	.56*	.24
Highest Grade Completed	.28**	.11
Number of Times Dropped Out of School	- .32*	.16
Number of Semesters in a Teen Parent School Program	.27+	.15
Pregnant at Baseline	.47+	.25
Employed at Baseline	.45	.41
Number of Jobs Held	.35**	.11
Date of Enrollment/Baseline	.00	.00
Participated in Project Redirection	.44*	.21
Constant	117.7	
Number of Respondents	652	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Teens who were either attending an educational program or had completed their high school or GED program, were working, or were looking for work at the final interview were coded 1; others were coded 0.

[b] Participation was coded 1 for experimental group teens, 0 for comparison group teens.

[c] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d] Unless otherwise specified, all explanatory variables are baseline characteristics.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.24

LOGISTIC REGRESSION OF EMPLOYMENT AT 24-MONTH INTERVIEW, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	(1)		(2)		(3)	
Age	- .01	(.13)[d]	.01	(.13)	- .03	(.14)
White	- .16	(.39)	- .14	(.39)	- .25	(.40)
Black	- .60*	(.28)	- .64*	(.28)	- .70**	(.28)
Puerto Rican	-1.03**	(.42)	-1.03**	(.42)	-1.01*	(.42)
Married	.58	(.42)	.65	(.42)	.42	(.43)
Raised by Both Parents	.12	(.27)	.10	(.27)	.10	(.27)
Number of Siblings	- .03	(.04)	- .03	(.04)	- .02	(.04)
In School or GED Program	.27	(.30)	.33	(.29)	.24	(.30)
Highest Grade Completed	.18	(.13)	.17	(.13)	.15	(.13)
Number of Times Dropped Out of School	- .02	(.20)	- .00	(.20)	.01	(.20)
Number of Semesters in a Teen Parent Program	.13	(.15)	.13	(.15)	.10	(.15)
Pregnant at Baseline	.41	(.31)	.43	(.32)	.45	(.32)
Age of Index Child	.03	(.02)	.03	(.02)	.03	(.02)
Employed at Baseline	.62+	(.34)	.66*	(.34)	.62+	(.34)
Number of Baseline Jobs	.09	(.12)	.08	(.12)	.07	(.12)
In an AFDC Household	- .20	(.25)	- .19	(.25)	- .13	(.26)
Date of Enrollment/Baseline	- .00	(.00)	.00	(.00)	.00	(.00)
Participated in Project Redirection	.16	(.25)	—		—	
Number of Months Participated in Project Redirection	—		.03	(.02)	—	
Received Job/Employability Training from Project Redirection	—		—		.82**	(.28)
Received Job/Employability Training Elsewhere Since Baseline	—		—		.80**	(.25)
Constant	39.67		-7.79		-10.92	
Number of Respondents	652		652		652	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES. [a] Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of job/employability training from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such training elsewhere during the follow-up period.

[b] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c] Unless otherwise specified, all explanatory variables are baseline characteristics.

[d] The coefficients shown are standardized coefficients (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.

TABLE E.25

ORDINARY LEAST SQUARES REGRESSION OF NUMBER OF JOBS EVER HELD AT 24-MONTH INTERVIEW, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	(1)		(2)		(3)	
Age at First Birth	- .10+	(.06)[d]	- .10+	(.06)	- .10	(.06)
White	.48*	(.26)	.50*	(.25)	.42+	(.24)
Black	- .11	(.16)	- .13	(.16)	- .15	(.16)
Puerto Rican	- .42*	(.20)	- .42*	(.20)	- .39+	(.20)
Raised by Both Parents	.04	(.16)	.02	(.15)	.01	(.15)
Number of Siblings	- .04+	(.02)	- .04+	(.02)	- .04+	(.02)
In School or GED Program	.20	(.16)	.20	(.16)	.12	(.16)
Highest Grade Completed	.23***	(.07)	.23***	(.07)	.22**	(.07)
Number of Semesters in a Teen Parent Program	.08	(.09)	.08	(.09)	.06	(.09)
Pregnant at Baseline	- .16	(.16)	- .15	(.16)	- .15	(.16)
Age of Youngest Child	.01	(.00)	.01	(.01)	.01	(.01)
Number of Baseline Jobs	.97***	(.07)	.96***	(.07)	.95***	(.07)
In an AFDC Household	.01	(.14)	.01	(.14)	.04	(.14)
Date of Enrollment/Baseline	- .00	(.00)	- .00	(.00)	- .00	(.00)
Participated in Project Redirection	.26*	(.13)	—		—	
Number of Months Participated in Project Redirection	—		.02*	(.01)	—	
Received Employment Training From Project Redirection	—		—		.44**	(.17)
Received Employment Training Elsewhere Since Baseline	—		—		.46***	(.13)
Constant	47.0		33.4		42.7	
Adjusted R^2	.34		.34		.35	
Number of Respondents	652		652		652	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of job/employability training from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such training elsewhere during the follow-up period.

[b] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c] Unless otherwise specified, all explanatory variables are baseline characteristics.

[d] The coefficients shown are unstandardized (b's). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.26

LOGISTIC REGRESSION OF EMPLOYMENT BETWEEN BASELINE AND 24-MONTH INTERVIEW, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	(1)		(2)		(3)	
Age	- .06	(.10)[d]	- .06	(.10)	- .10	(.10)
White	.09	(.36)	.11	(.36)	- .09	(.37)
Black	.20	(.22)	.17	(.22)	.09	(.23)
Puerto Rican	- .48+	(.27)	- .49+	(.27)	- .47+	(.28)
Married	.46	(.36)	.47	(.36)	.60+	(.37)
Raised by Both Parents	.10	(.22)	.07	(.22)	.01	(.23)
Number of Siblings	.00	(.03)	.00	(.03)	.01	(.03)
In School or GED Program	.75***	(.22)	.77***	(.22)	.62**	(.23)
Highest Grade Completed	.04	(.10)	.03	(.10)	.01	(.10)
Number of Times Dropped Out of School	- .05	(.15)	- .04	(.15)	- .05	(.16)
Number of Semesters in a Teen Parent Program	- .09	(.13)	- .10	(.13)	- .16	(.13)
Pregnant at Baseline	- .33	(.23)	- .32	(.23)	- .36	(.23)
Age of Index Child	.02	(.02)	.02	(.02)	.02	(.02)
Employed at Baseline	1.45***	(.43)	1.48***	(.43)	1.51***	(.44)
Number of Baseline Jobs	.32***	(.10)	.32***	(.10)	.30**	(.10)
In an AFDC Household	- .42*	(.20)	- .42*	(.20)	- .39+	(.20)
Date of Enrollment/Baseline	- .00	(.00)	.00	(.00)	.00	(.00)
Participated in Project Redirection	.32*	(.18)	—		—	
Number of Months Participated in Project Redirection	—		.03**	(.01)	—	
Received Job/Employability Training from Project Redirection	—		—		.91***	(.26)
Received Job/Employability Training Elsewhere Since Baseline	—		—		1.09***	(.19)
Constant	64.53		40.04		60.70	
Number of Respondents	652		652		652	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of job/employability training from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such training elsewhere during the follow-up period.

[b] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c] Unless otherwise specified, all explanatory variables are baseline characteristics.

[d] The coefficients shown are standardized coefficients (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.27

LOGISTIC REGRESSION OF WELFARE STATUS[a] AT 24-MONTH INTERVIEW, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1)		(2)	
Age	.04	(.11)[e]	.31***	(.10)
White	.29	(.35)	.25	(.33)
Black	.85***	(.23)	.29	(.21)
Puerto Rican	1.67***	(.35)	1.69***	(.31)
Married	-1.11**	(.40)	-1.03	(.38)
Age of Index Child	- .02	(.02)	- .00	(.01)
In an AFDC Household	1.12***	(.21)	.59***	(.19)
Raised by Both Parents	- .21	(.23)	.01	(.22)
Number of Siblings	.06+	(.03)	.04	(.03)
In School or GED Program	- .05	(.25)	- .25	(.22)
Highest Grade Completed	- .09	(.11)	- .01	(.09)
Number of Times Dropped Out of School	- .04	(.17)	- .04	(.15)
Number of Semesters in a Teen Parent School Program	.18	(.14)	- .04	(.12)
Pregnant at Baseline	- .17	(.26)	- .14	(.22)
Employed at Baseline	- .52	(.33)	- .53+	(.31)
Number of Jobs Held	.08	(.11)	.08	(.09)
Date of Enrollment/Baseline	.00+	(.00)	.00	(.00)
Participated in Project Redirection	.54*	(.22)	.22	(.19)
Constant	-186.9		-44.9	
Number of Respondents	652		652	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]In analysis (1), teens living in a household in which any member was receiving AFDC at the 24-month interview were coded 1; others were coded 0. In analysis (2), teens who reported having their own AFDC grant were coded 1; others were coded 0.

[b]Participation was coded 1 for experimental group teens, 0 for comparison group teens.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The coefficients shown are standardized coefficient (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.28

LOGISTIC REGRESSION OF EMPLOYMENT 12 MONTHS AFTER TERMINATION OF INDEX PREGNANCY, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	(1)		(2)		(3)	
Age	.07	(.14)[d]	.07	(.14)	.08	(.14)
White	.18	(.40)	.17	(.40)	.15	(.41)
Black	- .57+	(.30)	- .56+	(.30)	- .59*	(.30)
Puerto Rican	.02	(.36)	.03	(.36)	.06	(.36)
Married	.03	(.45)	.03	(.45)	.13	(.45)
Raised by Both Parents	.04	(.28)	.05	(.28)	.05	(.28)
Number of Siblings	.01	(.04)	.00	(.04)	.01	(.04)
In School or GED Program	.18	(.30)	.18	(.30)	.22	(.30)
Highest Grade Completed	.12	(.12)	.12	(.13)	.09	(.13)
Number of Times Dropped Out of School	.02	(.20)	.02	(.20)	.04*	(.20)
Number of Semesters in a Teen Parent Program	.10	(.16)	.10	(.16)	.10	(.16)
Number of Months Between Baseline and Index Pregnancy	.02	(.02)	.02	(.02)	.02	(.02)
Age of Index Child	- .04+	(.02)	- .04+	(.02)	- .04+	(.02)
Employed at Baseline	1.06**	(.34)	1.06**	(.34)	1.07**	(.34)
Number of Baseline Jobs	.06	(.12)	.06	(.12)	.04	(.12)
In an AFDC Household	- .48+	(.26)	- .48+	(.26)	- .45	(.26)
Date of Enrollment/Baseline	- .00*	(.00)	- .00*	(.00)	- .00*	(.00)
Participated in Project Redirection	- .15	(.26)	—		—	
Number of Months Participated in Project Redirection	—		- .01	(.02)	—	
Received Job/Employability Training From Project Redirection	—		—		.31	(.31)
Received Job/Employability Training Elsewhere Since Baseline	—		—		.27	(.25)
Constant	257.39		265.48		215.78	
Number of Respondents	646		646		646	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of job/employability training from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such training elsewhere during the follow-up period.

[b]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c]Unless otherwise specified, all explanatory variables are baseline characteristics.

[d]The coefficients shown are standardized coefficients (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.

TABLE E.29

LOGISTIC REGRESSION OF EMPLOYMENT 24-MONTHS AFTER TERMINATION OF INDEX PREGNANCY, ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[a]

Explanatory Variable[b,c]	(1)		(2)		(3)	
Age	.08	(.18)[d]	.08	(.18)	.10	(.18)
White	-1.96+	(.56)	-1.01+	(.56)	-1.13*	(.57)
Black	- .92**	(.34)	- .94**	(.34)	- .99**	(.34)
Puerto Rican	-1.93**	(.67)	-1.94**	(.67)	-1.97**	(.67)
Married	- .20	(.73)	- .10	(.72)	- .04	(.72)
Raised by Both Parents	- .05	(.35)	- .05	(.35)	- .10	(.35)
Number of Siblings	- .04	(.05)	- .04	(.05)	- .04	(.05)
In School or GED Program	.40	(.37)	.47	(.37)	.40	(.37)
Highest Grade Completed	.15	(.16)	.14	(.16)	.09	(.16)
Number of Times Dropped Out of School	- .13	(.25)	- .12	(.25)	- .10	(.26)
Number of Semesters in a Teen Parent Program	- .21	(.18)	- .19	(.18)	- .17	(.19)
Number of Months Between Baseline and Index Pregnancy	- .01	(.03)	- .01	(.03)	- .01	(.03)
Age of Index Child	.01	(.02)	.01	(.02)	.02	(.03)
Employed at Baseline	.58	(.45)	.57	(.44)	.58	(.45)
Number of Baseline Jobs	.34*	(.15)	.35*	(.15)	.33*	(.15)
In an AFDC Household	- .56+	(.33)	- .55+	(.33)	- .53	(.34)
Date of Enrollment/Baseline	- .00	(.00)	- .00	(.00)	- .00	(.00)
Participated in Project Redirection	- .22	(.33)	—		—	
Number of Months Participated in Project Redirection	—		- .01	(.02)	—	
Received Job/Employability Training From Project Redirection	—		—		- .31	(.42)
Received Job/Employability Training Elsewhere Since Baseline	—		—		.46	(.32)
Constant	135.24		103.72		69.97	
Number of Respondents	345		345		345	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of educational counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[b] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c] Unless otherwise specified, all explanatory variables are baseline characteristics.

[d] The coefficients shown are standardized coeffieients (Betas). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.

TABLE E.30

ORDINARY LEAST SQUARES REGRESSION OF EMPLOYABILITY KNOWLEDGE TEST SCORES[a] FOR SAMPLE I TEENS AT 24-MONTH INTERVIEW, ON BASELINE CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1)		(2)		(3)	
Age	- .05	(.17)[e]	- .14	(.17)	- .17	(.17)
White	.70	(.55)	.65	(.56)	.65	(.57)
Black	.54	(.37)	.58	(.38)	.60	(.38)
Puerto Rican	-1.46**	(.50)	-1.45**	(.51)	-1.56**	(.52)
Married	- .30	(.63)	- .45	(.63)	- .42	(.64)
Raised by Both Parents	- .15	(.35)	- .16	(.36)	- .14	(.36)
Number of Siblings	- .03	(.04)	- .05	(.05)	- .05	(.05)
Mother's Education	- .05*	(.03)	- .06*	(.03)	- .05*	(.03)
Father's Education	.10**	(.03)	.10**	(.03)	.10	(.03)
In School or GED Program	- .23	(.38)	- .45	(.38)	- .52	(.37)
Highest Grade Completed	- .10	(.17)	- .02	(.17)	.03	(.18)
Number of Times Dropped out of School	- .15	(.23)	- .19	(.24)	- .20	(.24)
Number of Semesters in a Teen Parent Program	.50*	(.20)	.35+	(.20)	.35	(.20)+
Pregnant at Baseline	.35	(.31)	- .40	(.32)	- .36	(.32)
Employed at Baseline	.64	(.51)	.60	(.52)	.62	(.53)
Number of Baseline Jobs	.01	(.14)	.08	(.15)	.03	(.15)
In an AFDC Household	- .09	(.35)	- .05	(.36)	- .15	(.36)
Baseline Scores, Employability Knowledge Test	.49***	(.05)	.49***	(.05)	.50***	(.05)
Baseline Scores, Career Maturity Test	.13***	(.03)	.14***	(.04)	.14***	(.04)
Participated in Project Redirection	1.04***	(.35)	—		—	
Number of Months Participated in Project Redirection	—		.06**	(.02)	—	
Received Job/Employablility Training From Project Redirection	—		—		.43	(.45)
Received Job/Employability Training Elsewhere Since Baseline	—		—		.21	(.32)
Constant	5.19		5.55		6.92	
Adjusted R^2	.45		.45		.44	
Number of Respondents	359		359		359	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I only at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] The Employability Knowledge Test consists of 17 items, each of which is scored as 1 point if it is answered correctly. Higher scores reflect greater knowledge.

[b] Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable was included for receipt of job/employability training from the program, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such training elsewhere during the follow-up period.

[c] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d] Unless otherwise specified, all explanatory variables are baseline characteristics.

[e] The coefficients shown are unstandardized (b's). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.31

ORDINARY LEAST SQUARES REGRESSION OF CAREER MATURITY TEST SCORES[a] FOR SAMPLE I TEENS AT 24-MONTH INTERVIEW, ON BASELINE CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1)		(2)		(3)	
Age	- .33	(.23)[e]	- .32	(.24)	- .32	(.23)
White	1.11	(.76)	1.09	(.78)	1.03	(.78)
Black	- .75	(.52)	- .73	(.53)	- .88	(.52)
Puerto Rican	- .72	(.70)	- .70	(.72)	- .59	(.71)
Married	- .74	(.88)	- .71	(.88)	- .33	(.87)
Raised by Both Parents	.24	(.49)	.26	(.49)	.18	(.49)
Number of Siblings	.02	(.06)	.02	(.06)	.04	(.06)
Mother's Education	- .01	(.04)	- .01	(.04)	- .01	(.03)
Father's Education	- .01	(.05)	- .00	(.05)	- .00	(.05)
In School at Baseline	-1.41**	(.52)	-1.37**	(.52)	-1.25*	(.51)
Highest Grade Completed	.48*	(.24)	.45+	(.24)	.42+	(.24)
Number of Times Dropped out of School	- .47	(.33)	- .47	(.33)	- .38	(.32)
Number of Semesters in a Teen Parent Program	.12	(.28)	.11	(.28)	.14	(.28)
Pregnant at Baseline	- .30	(.44)	- .31	(.45)	- .21	(.44)
Employed at Baseline	.86	(.72)	.83	(.73)	.66	(.72)
Number of Baseline Jobs	- .14	(.20)	- .16	(.20)	- .20	(.20)
In an AFDC Household	- .21	(.50)	- .22	(.50)	- .12	(.50)
Baseline Scores, Employability Knowledge Test	.33***	(.07)	.32***	(.07)	.29***	(.07)
Baseline Scores, Career Maturity Test	.47***	(.05)	.48***	(.05)	47***	(.05)
Participated in Project Redirection	- .42	(.49)	—		—	
Number of Months Participated in Project Redirection	—		- .02	(.03)	—	
Received Job/Employability Training From Project Redirection	—		—		.74	(.60)
Received Job/Employability Training Elsewhere Since Baseline	—		—		1.15**	(.43)
Constant	13.5		13.3		13.0	
Adjusted R^2	.42		.43		.43	
Number of Respondents	359		359		359	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I only at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]The Career Maturity Inventory consists of 30 items, each of which is scored as 1 point if it is answered correctly. Higher scores reflect greater career maturity.

[b]Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable was included for receipt of job/employability training from the program, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such training elsewhere during the follow-up period.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The coefficients shown are unstandardized (b's). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.32

ORDINARY LEAST SQUARES REGRESSION OF SCORES ON ATTITUDE TOWARD NONTRADITIONAL EMPLOYMENT SCALE[a] FOR SAMPLE I TEENS AT 24 MONTH INTERVIEW, ON BASELINE CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1)		(2)	
Age	- .05	(.14)[e]	- .06	(.14)
White	.94*	(.45)	.93*	(.45)
Black	- .45	(.32)	- .43	(.32)
Puerto Rican	-1.03*	(.41)	-1.03*	(.41)
Married	- .21	(.51)	- .23	(.51)
Raised by Both Parents	- .35	(.29)	- .34	(.29)
Number of Siblings	.01	(.04)	.01	(.04)
Mother's Education	.13*	(.06)	.12*	(.06)
Father's Education	.02	(.05)	.02	(.05)
In School or GED Program	- .31	(.30)	- .33	(.30)
Highest Grade Completed	.02	(.14)	.02	(.14)
Number of Times Dropped Out of School	.12	(.19)	.11	(.19)
Number of Semesters in a Teen Parent Program	.12	(.16)	.12	(.16)
Employed at Baseline	.40	(.42)	.40	(.42)
Number of Baseline Jobs	.08	(.12)	.09	(.12)
In an AFDC Household	.14	(.29)	.14	(.29)
Pregnant at Baseline	.16	(.26)	.14	(.26)
Baseline Scores, Nontraditional Employment Scale	.40***	(.05)	.41***	(.05)
Participated in Project Redirection	- .03	(.26)	—	
Number of Months Participated in Project Redirection	—		- .01	(.05)
Constant	9.24		9.38	
Adjusted R^2	.22		.22	
Number of Respondents	343		343	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I only at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] The scale was a five-item Likert Scale that measured attitudes toward women's employment in jobs traditionally performed by men. The scores could range from a low of five (negative attitudes toward nontraditional employment) to a high of 20 (positive attitudes toward nontraditional employment).

[b] Participation was examined in two ways. In analysis (1) participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members).

[c] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d] Unless otherwise specified, all explanatory variables are baseline characteristics.

[e] The coefficients shown are unstandardized (b's). The numbers in parentheses are the standard errors.

*Statistically significant at the .05 level.
***Statistically significant at the .001 level.

TABLE E.33

ORDINARY LEAST SQUARES REGRESSION OF SELF-ESTEEM SCORES[a] FOR SAMPLE I TEENS AT 24-MONTH INTERVIEW, ON BASELINE CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1)		(2)	
Age	.13	(.15)[e]	.13	(.15)
White	.19	(.51)	.23	(.50)
Black	.68+	(.37)	.63	(.37)
Puerto Rican	.41	(.46)	.40	(.46)
Married	- .55	(.57)	- .55	(.57)
Raised by Both Parents	.07	(.33)	.01	(.33)
Number of Siblings	- .01	(.04)	- .01	(.04)
Mother's Education	.10	(.07)	.11	(.07)
Father's Education	- .06	(.06)	- .06	(.06)
In School or GED Program	.17	(.34)	.16	(.34)
Highest Grade Completed	.02	(.16)	.04	(.15)
Number of Times Dropped Out of School	- .04	(.21)	- .05	(.22)
Number of Semesters in a Teen Parent Program	- .16	(.18)	- .20	(.18)
Employed at Baseline	.76	(.48)	.78	(.48)
Number of Jobs Held at Baseline	.06	(.13)	.06	(.13)
In an AFDC Household	.31	(.33)	.33	(.33)
Pregnant at Baseline	- .01	(.29)	.02	(.29)
Baseline Scores, Self Esteem Scale	.41***	(.06)	.41***	(.06)
Participated in Project Redirection	.59*	(.29)	—	
Number of Months Participated in Project Redirection	—		.04*	(.02)
Constant	8.50		8.50	
Adjusted R^2	.19		.20	
Number of Respondents	343		343	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I only at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a] The Self-Esteem Scale was a six-item scale designed to measure feelings of self worth. Scores could range from six (low self-esteem) to 24 (high self-esteem).

[b] Participation was examined in two ways. In analysis (1) participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members).

[c] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d] Unless otherwise specified, all explanatory variables are baseline characteristics.

[e] The coefficients shown are unstandardized (b's). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
***Statistically significant at the .001 level.

TABLE E.34

ORDINARY LEAST SQUARES REGRESSION OF LOCUS OF CONTROL SCORES[a] FOR SAMPLE I TEENS AT 24 MONTH INTERVIEW, ON BASELINE CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1)		(2)	
Age	.00	(.13)[e]	.00	(.13)
White	1.02*	(.42)	1.05*	(.42)
Black	-.15	(.30)	-.18	(.30)
Puerto Rican	-.83*	(.39)	-.85*	(.39)
Married	.36	(.48)	.36	(.48)
Raised by Both Parents	.19	(.27)	.15	(.27)
Number of Siblings	-.01	(.04)	-.01	(.04)
Mother's Education	.18**	(.06)	.18**	(.06)
Father's Education	.06	(.05)	.06	(.05)
In School or GED Program	-.47	(.28)	-.49+	(.28)
Highest Grade Completed	.02	(.13)	.02	(.13)
Number of Times Dropped Out of School	-.05	(.18)	-.06	(.18)
Number of Semesters in a Teen Parent Program	.18	(.15)	.15	(.15)
Employed at Baseline	.60	(.40)	.61	(.40)
Number of Baseline Jobs	.10	(.11)	.10	(.11)
In an AFDC Household	.70*	(.28)	.71*	(.27)
Pregnant at Baseline	.29	(.24)	.31	(.24)
Baseline Scores, Locus of Control Scale	.47***	(.06)	.47***	(.06)
Participated in Project Redirection	.47+	(.25)	—	
Number of Months Participated in Project Redirection	—		.03+	(.02)
Constant	6.66		6.76	
Adjusted R^2	.29		.29	
Number of Respondents	343		343	

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I only at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: [a]The Locus-of-Control Scale was a five-item scale designed to measure feelings of personal control. Scores could range from five (designating an external control orientation) to 20 (designating an internal control orientation).

[b]Participation was examined in two ways. In analysis (1) participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members).

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The coefficients shown are unstandardized (b's). The numbers in parentheses are the standard errors.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.35

ORDINARY LEAST SQUARES REGRESSION OF LENGTH OF PARTICIPATION IN PROJECT REDIRECTION ON BACKGROUND CHARACTERESTICS[a]

Explanatory Variable[b]	Unstandardized Coefficient	Standard Error
Black	2.40+	1.26
Hispanic	0.74	1.28
Age	-0.94*	.39
Age at First Birth	0.42*	.19
Number of Siblings	-0.19+	.11
In an AFDC Household at Baseline	0.50	.78
Ever Worked at Baseline	2.43**	.79
Pregnant at Baseline	-0.37	.77
Date Enrolled in Redirection	-0.00***	.00
Constant	88.96	
Adjusted R^2	.37	
Number of Respondents	303	

SOURCE: Tabulations are from AIR interviews with experimental group members in Samples I and II at baseline, 12-months after baseline, and 24-month after baseline.

NOTES: [a] Length of participation is measured in number of months.

[b] All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

+Statistically significant at the .10 level
*Statistically significant at the .05 level
**Statistically significant at the .01 level
***Statistically significant at the .001 level

TABLE E.36

ADJUSTED PERCENTAGES OF TEENS IN SCHOOL OR A GED PROGRAM OR COMPLETED SCHOOL AT 12-MONTH INTERVIEW, FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Adjusted Percentages, by Group		
		Experimental	Comparison	Difference
Black Teens	(N=313)	68	64	4
Mexican American Teens	(N=163)	52	30	22**
Puerto Rican Teens	(N=119)	31	19	12
Teens Age 15 or Younger at Baseline	(N=224)	60	50	10
Teens Age 16-17 at Baseline	(N=450)	57	45	12**
Teens Pregnant at Baseline	(N=416)	62	46	16***
Teens Not Pregnant at Baseline	(N=258)	53	47	6
Teens in an AFDC Household at Baseline	(N=428)	54	45	9*
Teens not in an AFDC Household at Baseline	(N=230)	65	53	12*
Teens in School at Baseline	(N=375)	72	67	5
Teens Not in School at Baseline	(N=299)	40	20	20***
Boston/Hartford Teens	(N=114)	31	19	12
Harlem/Bedford Stuyvesant Teens	(N=186)	58	63	- 5
Phoenix/San Antonio Teens	(N=289)	67	44	23***
Riverside/Fresno Teens	(N= 85)	54	63	- 9
Sample I Teens	(N=385)	60	48	12*
Sample II Teens	(N=289)	55	46	9+
All Teens	(N=674)	56	49	7*

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The percentage have been adjusted for ethnicity, school status at baseline, highest grade completed at baseline, number of times dropped out of school at baseline, enrollment in a teen parent program at baseline, number of baseline pregnancies, and age at first birth.

*Statistically significant at the .05 level.

TABLE E.37

ADJUSTED PERCENTAGES OF TEENS EVER ENROLLED IN SCHOOL BETWEEN BASELINE AND 12-MONTH INTERVIEW, FOR TEENS IN VARIOUS SUBGROUPS

Subgroup	Adjusted Percentages, by Group		
	Experimental	Comparison	Difference
Black Teens (N=313)	79	56	23***
Mexican American Teens (N=163)	77	53	24***
Puerto Rican Teens (N=119)	69	31	37***
Teens Age 15 or Younger at Baseline (N=224)	73	59	14**
Teens Age 16-17 at Baseline (N=450)	74	50	24***
Teens Pregnant at Baseline (N=416)	77	55	22***
Teens Not Pregnant at Baseline (N=258)	72	48	24***
Teens in an AFDC Household at Baseline (N=441)	71	54	17***
Teens not in an AFDC Household at Baseline (N=231)	82	50	32***
Teens in School at Baseline (N=375)	86	75	11**
Teens Not in School at Baseline (N=299)	56	23	33***
Boston/Hartford Teens (N=114)	68	35	33***
Harlem/Bedford Stuyvesant Teens (N=186)	60	44	16**
Phoenix/San Antonio Teens (N=289)	85	57	28***
Riverside/Fresno Teens (N= 95)	70	74	- 4
Sample I Teens (N=385)	73	60	13**
Sample II Teens (N=289)	77	41	36***
All Teens (N=674)	75	51	24***

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Sample I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The percentage have been adjusted for ethnicity, school status at baseline, highest grade completed at baseline, age at first birth, enrollment in a teen parent program at baseline, number of baseline pregnancies, and number of jobs held at baseline.

**Statistically significant at the .01 level.
***Statistically significant at the .001 level.

TABLE E.38

ADJUSTED PERCENTAGES OF TEENS WITH WORK EXPERIENCE BETWEEN BASELINE AND 12-MONTH INTERVIEW, FOR TEENS IN VARIOUS SUBGROUPS

Subgroup		Adjusted Percentages, by Group		
		Experimental	Comparison	Difference
Black Teens	(N=306)	52	42	10+
Mexican American Teens	(N=160)	54	39	15*
Puerto Rican Teens	(N=118)	38	17	21**
Teens Age 15 or Younger at Baseline	(N=218)	43	27	16**
Teens Age 16-17 at Baseline	(N=442)	53	41	12**
Teens Pregnant at Baseline	(N=410)	44	34	10*
Teens Not Pregnant at Baseline	(N=250)	57	41	16**
Teens in an AFDC Household at Baseline	(N=428)	48	33	15***
Teens not in an AFDC Household at Baseline	(N=230)	53	44	9
Teens in School at Baseline	(N=370)	54	46	8
Teens not in School at Baseline	(N=290)	43	23	20***
Teens With Work Experience at Baseline	(N=411)	56	46	10*
Teens Without Work Experience at Baseline	(N=249)	41	21	20***
Boston/Hartford Teens	(N=113)	40	17	23**
Harlem/Bedford Stuyvesant Teens	(N=181)	39	39	0
Phoenix/San Antonio Teens	(N=181)	63	44	19**
Riverside/Fresno Teens	(N= 84)	42	29	13
Sample I Teens	(N=376)	53	36	17***
Sample II Teens	(N=284)	47	36	11*
All Teens	(N=660)	49	38	11**

SOURCE: Tabulations are from AIR interviews with experimental and comparison group members in Samples I and II at baseline, 12 months after baseline, and 24 months after baseline.

NOTES: The percentages shown have been adjusted for ethnicity, age of the youngest child, pregnancy status at baseline, number of baseline pregnancies, school status at baseline, AFDC status at baseline, number of semesters repeated at baseline and number of jobs held at baseline.

+Statistically significant at the .10 level.
*Statistically significant at the .05 level.
**Statistically significant at the .001 level.

APPENDIX F

REFERENCES

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REFERENCES

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APPENDIX G

GLOSSARY

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Because this document contains many technical terms and features idiosyncratic to the design of this study, this glossary of terms has been prepared to assist readers who may either be unfamiliar with terminology or who may wish to refresh their memories concerning certain aspects of the design. The entries are in alphabetic order; some entries also direct the reader to the portions of the document that provide a more detailed discussion of the term.

AFDC Aid to families with dependent children, the federally sponsored welfare program. The majority of participants in this study were AFDC recipients.

AIR American Institutes for Research in the Behavioral Sciences, the research organization responsible for the collection of three waves of impact analysis data.

Attrition biases Biases that could result from differences between respondents who did and did not complete follow-up interviews. See Chapter 2 and Appendix C.

Baseline The point of the initial data collection, prior to any programmatic intervention. See Chapter 2.

Bedford-Stuyvesant site A comparison site located in Brooklyn, New York, chosen to be the match for the experimental site in Harlem.

Boston site One of the Project Redirection demonstration sites, serving primarily Puerto Rican teens. The sponsoring agency was El Centro del Cardinal (Cardinal Cushing Center).

Community woman A feature of the Project Redirection demonstration model that involved a woman drawn from the participants' community to serve as a primary support to the teens, analagous to a "big sister". See Chapter 1.

Comparison teens Teens not participating in Project Redirection but meeting eligibility criteria; these teens were drawn from four sites (Hartford, Bedford-Stuyvesant (NY), San Antonio, and Fresno (CA), matched to demonstration sites. Many comparison teens were obtaining special services (but not Redirection services) in their own communities.

Detroit site One of the initial Project Redirection demonstration sites, operating through the Urban League. The Detroit program was discontinued in 1981 due to management difficulties.

Fresno site A comparison site, chosen to be the match for the experimental site in Riverside, California.

GED General Equivalency Diploma, the equivalent of a high school diploma obtained by passing a special examination.

Exit criteria Criteria established during Phase II of the Project Redirection demonstration that mandated a participant's termination from the program. The exit criteria were (1) completion of high school diploma or GED certificate; (2) enrollment for 18 months; or (3) attainment of the 19th birthday.

Experimental teens Teens from Boston, Harlem, Phoenix and Riverside (CA) who were enrolled in the Project Redirection program for at least 30 days and who were interviewed by AIR for this impact analysis study.

Harlem site One of the Project Redirection demonstration sites, serving primarily black teens. The sponsoring agency was the Harlem YMCA.

Hartford site A comparison site, chosen to be the match for the experimental site in Boston.

Index pregnancy The pregnancy in progress at baseline (for teens pregnant at baseline) or the most recently terminated pregnancy at baseline (for teen mothers at baseline). See Chapter 4.

IPP Individual Participant Plan, a planning and monitoring tool developed collaboratively by program staff, the teen, and her community woman. The IPP specified the teen's goals and the plan of services designed to her achieving them. See Chapter 1.

Labor force participation An outcome measure, defined as a teen being employed or seeking employment at a specific point in time, based on the teen's self report. See Chapter 6.

Lottery The technique used to encourage continuity of contact between comparison teens and the research staff between interviews. Returned postcards indicating respondents' whereabouts were considered entries into a yearly lottery, with prizes distributed in each comparison site. See Chapter 2.

Low birthweight infant An outcome measure, defined as an infant weighing under 2500 grams (5.5 pounds) at birth.

MDRC Manpower Demonstration Research Corporation, a nonprofit organization that designs and evaluates innovative social programs. MDRC assisted the sites in implementing the program model and monitored local operations. The organization also bore overall responsibility for the research.

NLS National Longitudinal Survey of Labor Market Experiences, an annual survey of a national sample of men and women focusing primarily on respondents' employment and related activities; the survey is conducted by Ohio State University. See Chapter 7.

OAPP	The Office of Adolescent Pregnancy Programs, a federal office within the Department of Health and Human Services; OAPP sponsors various agencies nationwide that provide services to pregnant and parenting teens. See Chapter 3.
Phase I	The initial phase of program operation of the Project Redirection Demonstration that ran from June, 1980 to December, 1981; during this period programs were initiated in five sites (Boston, MA; Harlem, NY; Phoenix, AZ; Riverside, CA; and Detroit, MI). The Detroit program was discontinued in 1981 due to management difficulties. This report describes participants in the remaining four sites. See Chapter 1.
Phase II	The "mature" phase of program operations of Project Redirection that ran from January to December, 1982. During this phase more explicit guidelines for service delivery and exit criteria were provided to the sponsoring agencies by MDRC. See Chapter 1.
Phase III	The final phase of operations of the demonstration that served as a transition year (January to December, 1983) during which sponsoring agencies sought to identify alternative sources of funding. See Chapter 1.
Phoenix site	One of the Project Redirection demonstration sites, serving Chicana, black, and white teens primarily in South Phoenix. The sponsoring agency was Chicanos por la Causa, a community development corporation.
Postive school status	An outcome measure defined as attendance in an educational program or attainment of a high school diploma or GED certificate. See Chapter 5.
Postpartum	The period following termination of the index pregnancy. See Chapter 4.
Retrospective baseline data	For Sample II respondents, measures of baseline characteristics based on retrospective reports at the initial interview (12 months post-baseline). See Appendix A.
Riverside site	One of the Project Redirection demonstration sites, serving white, Chicana, and black teens in Riverside, California and its environs. The sponsoring agency was The Children's Home Society.
Sample I	Teens in the experimental and comparison groups who were administered a baseline interview between September, 1980 and March, 1981 (July, 1981 in Riverside). These teens were re-interviewed at 12 and 24 months after the baseline interview. Experimental teens in Sample I were interviewed within about 45 days of enrollment in Project Redirection. See Chapter 2 and Appendix A.

Sample II A supplemental sample added after completion of the initial wave of baseline interviews. These teens, for whom there is no baseline interview, were initially interviewed at 12 months post enrollment, from March, 1982 to March, 1983, and were re-interviewed 12 months later. Baseline data were developed retrospectively based on reports in the 12-month interview. The date of baseline was the date of enrollment for experimental teens and 12 months prior to the initial interview for comparison teens. See Chapter 2 and Appendix A.

San Antonio site A comparison site, chosen to be the match for the experimental site in Phoenix.

Selection bias A bias that could result from pre-intervention differences in the characteristics of the experimental and comparison group teens. See Chapter 2 and Appendix B.

Stipend, participant A monthly of about $30, paid to participants in Project Redirection during Phase I of operations through the Work Incentive Program (WIN). During Phase II, payment of the monthly stipend was discontinued.

Stipend, subject A payment of $10 for each completed interview, paid to comparison group teens only. See Chapter 2.

Subsequent pregnancy Any pregnancy that began after the termination of the index pregnancy. See Chapter 4.

24-month interview The follow-up interview scheduled 24 months after baseline; the third interview for Sample I, and the second interview for Sample II. See Chapter 2.

OTHER MDRC STUDIES OF PROJECT REDIRECTION

Needs and Characteristics of Pregnant and Parenting Teens: The Baseline Report for Project Redirection. Polit, Denise F.; with Kahn, Janet R.; Murray, Charles A.; and Smith, Kevin W. 1982.

School, Work and Family Planning: Interim Impacts in Project Redirection. Polit, Denise F.; Tannen, Michael S.; and Kahn, Janet R. 1983.

Choices and Life Circumstances: An Ethnographic Study of Project Redirection Teens. Levy, Sydelle Brooks; with Grinker, William J. 1983.

Project Redirection: Interim Report on Program Implementation. Branch, Alvia; Quint, Janet; with Mandel, Sheila; and Shuping Russell, Sallie. 1981.

Building Self-Sufficiency in Pregnant and Parenting Teens: Final Implementation Report of Project Redirection. Branch, Alvia; Riccio, James; and Quint, Janet. 1984.

The Challenge of Serving Pregnant and Parenting Teens: Lessons from Project Redirection. Riccio, James; and Quint, Janet. (A monograph summarizing the research findings and operational experience.) 1985.

The Replication of Project Redirection. (Forthcoming, late 1985.)

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